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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

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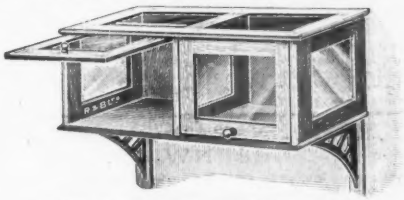

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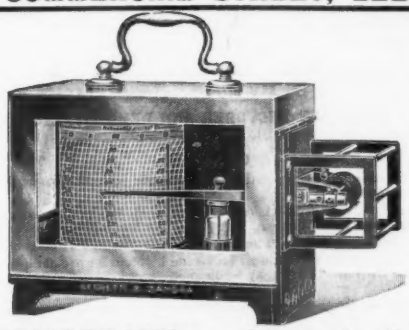

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UNIVERSITY OF LONDON.

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Introduction.

SINCE 1905, when Prof. A. Einstein published his Principle of Relativity in its special form, much attention has been devoted to the subject in scientific circles, and during the last two or three years keen interest has been shown in it by many general readers of intellectual type. Nearly a thousand books, pamphlets, and papers have appeared in which the principle as a whole is described, or some of its aspects are discussed, and among these are a few which aim at making its character and consequences clear without the mathematical expressions which give it precise significance. A praiseworthy essay of this kind is that by Mr. L. Bolton, a senior examiner in the Patent Office, published in the *Westminster Gazette* on Monday by arrangement with the *Scientific American*, which awarded it a prize of five thousand dollars as the clearest explanation of Einstein's principle for general readers.

It is not difficult to understand why such wide interest is taken in this principle. No special knowledge is required to realise that measurements of space and time are essentially relative, and as all thinking people have pondered over the metaphysics of infinity and eternity, they are attracted by a conception in which these ideas are involved. The physicist is concerned with the principle because it developed out of experimental results of a negative kind, and is intimately associated with electromagnetic theory; the astronomer because it gives a new interpretation of effects not explained by the Newtonian law; and the mathematician because it provides him with a new space-time geometry. The principle has thus points of contact with many fields of scientific activity, and it is on this account that the present issue of NATURE is devoted to it.

We are gratified that so many leading authorities have been able to favour readers of this journal with surveys of the foundations of the principle or with their views as to the stability of the framework based upon them. Our own function has been limited to suggesting the scope of each article in the series, and, so far as possible, securing that the range of the whole covers the chief points around which discussion has centred. The writers have not seen one another's contributions, so that each article is an independent statement complete in itself so far as it goes. The order of the articles in the series is, therefore, important, and we believe that adopted will be considered appropriate to these columns.

Prof. Einstein describes the natural sequence of ideas which led to the conception of his principle, and Mr. Cunningham follows with a historical sketch of the conditions which demanded a revision of æther theories in relation to problems of absolute motion. The astronomical consequences—the displacement of light by the gravitational field of the sun, the movement of the perihelion of Mercury's orbit, and the displacement of solar spectrum lines (not yet established)—are dealt with by Sir Frank Dyson, Dr. Crommelin, and Dr. St. John respectively. The relation of Riemann's geometry of n-dimensions to the principle is outlined by Prof. Mathews; and the four articles which follow, by Mr. Jeans, Prof. Lorentz, Sir Oliver Lodge, and Prof. Weyl, are concerned mainly with physical aspects. How differently philosophers and astronomers regard the meaning and measurement of time is described by Prof. Eddington, and this article, with those by Dr. Norman Campbell and by Miss Wrinch and Dr. Jeffreys, leads naturally to the metaphysical conceptions presented by Prof. Wildon Carr.

Whatever may be the ultimate place taken by the principle of relativity in the history of science, no idea has ever proceeded by more logical steps from the rank of hypothesis to theory. In two cases predicted phenomena for which no satisfactory alternative explanation is forthcoming have been confirmed by observation, and the third is still a subject of inquiry. In this journal we are concerned mainly with the bearing of the principle upon physical science, and only incidentally with its metaphysical aspects. We may remark, however, that the absolute in Nature is not abolished by the principle. Measurements of time and space cease to be absolute and depend upon the motion of the observer, but things like energy and the velocity of light are independent of such motion and remain as absolute as ever they were.

A Brief Outline of the Development of the Theory of Relativity.

By PROF. A. EINSTEIN.

[Translated by Dr. Robert W. Lawson.]

THERE is something attractive in presenting the evolution of a sequence of ideas in as brief a form as possible, and yet with a completeness sufficient to preserve throughout the continuity of development. We shall endeavour to do this for the Theory of Relativity, and to show that the whole ascent is composed of small, almost self-evident steps of thought.

The entire development starts off from, and is dominated by, the idea of Faraday and Maxwell, according to which all physical processes involve a continuity of action (as opposed to action at a distance), or, in the language of mathematics, they are expressed by partial differential equations. Maxwell succeeded in doing this for electro-magnetic processes in bodies at rest by means of the conception of the magnetic effect of the vacuum-displacement-current, together with the postulate of the identity of the nature of electro-dynamic fields produced by induction, and the electro-static field.

The extension of electro-dynamics to the case of moving bodies fell to the lot of Maxwell's successors. H. Hertz attempted to solve the problem by ascribing to empty space (the æther) quite similar physical properties to those possessed by ponderable matter; in particular, like ponderable matter, the æther ought to have at every point a definite velocity. As in bodies at rest, electro-magnetic or magneto-electric induction ought to be determined by the rate of change of the electric or magnetic flow respectively, provided that these velocities of alteration are referred to surface elements moving with the body. But the theory of Hertz was opposed to the fundamental experiment of Fizeau on the propagation of light in flowing liquids. The most obvious extension of Maxwell's theory to the case of moving bodies was incompatible with the results of experiment.

At this point, H. A. Lorentz came to the rescue. In view of his unqualified adherence to the atomic theory of matter, Lorentz felt unable to regard the latter as the seat of continuous electro-magnetic fields. He thus conceived of these fields as being conditions of the æther, which was regarded as continuous. Lorentz considered the æther to be intrinsically independent of matter, both from a mechanical and a physical point of view. The æther did not take part in the motions of matter, and a reciprocity between æther and matter could be assumed only in so far as the latter was considered to be the carrier of attached electrical charges. The great value of the theory of Lorentz lay in the fact that the entire electro-dynamics of bodies at rest and of bodies in motion was led back to Maxwell's equations of empty space. Not only did this theory surpass that of Hertz from the point of view of method, but with its aid H. A. Lorentz was also pre-eminently successful in explaining the experimental facts.

The theory appeared to be unsatisfactory only in *one* point of fundamental importance. It appeared to give preference to one system of co-ordinates of a particular state of motion (at rest relative to the æther) as against all other systems of co-ordinates in motion with respect to this one. In this point the theory seemed to stand in direct opposition to classical mechanics, in which all inertial systems which are in uniform motion with respect to each other are equally justifiable as systems of co-ordinates (Special Principle of Relativity). In this connection, all experience also in the realm of electro-dynamics (in particular Michelson's experiment) supported the idea of the equivalence of all inertial systems, *i.e.* was in favour of the special principle of relativity.

The Special Theory of Relativity owes its origin to this difficulty, which, because of its fundamental nature, was felt to be intolerable. This theory originated as the answer to the question: Is the special principle of relativity really contradictory to the field equations of Maxwell for empty space? The answer to this question appeared to be in the affirmative. For if those equations are valid with reference to a system of co-ordinates K, and we introduce a new system of co-ordinates K′ in conformity with the—to all appearances readily establishable—equations of transformation

$$\left.\begin{aligned} x' &= x - vt \\ y' &= y \\ z' &= z \\ t' &= t \end{aligned}\right\} \text{(Galileo transformation)},$$

then Maxwell's field equations are no longer valid in the new co-ordinates (x', y', z', t'). But appearances are deceptive. A more searching analysis of the physical significance of space and time rendered it evident that the Galileo transformation is founded on arbitrary assumptions, and in particular on the assumption that the statement of simultaneity has a meaning which is independent of the state of motion of the system of co-ordinates used. It was shown that the field equations for *vacuo* satisfy the special principle of relativity, provided we make use of the equations of transformation stated below:

$$\left.\begin{aligned} x' &= \frac{x - vt}{\sqrt{1 - v^2/c^2}} \\ y' &= y \\ z' &= z \\ t' &= \frac{t - vx/c^2}{\sqrt{1 - v^2/c^2}} \end{aligned}\right\} \text{(Lorentz transformation)}.$$

In these equations x, y, z represent the co-ordinates measured with measuring-rods which are at rest with reference to the system of co-ordinates, and t represents the time measured with suitably adjusted clocks of identical construction, which are in a state of rest.

Now in order that the special principle of relativity may hold, it is necessary that all the equations of physics do not alter their form in the transition from one inertial system to another, when we make use of the Lorentz transformation for the calculation of this change. In the language of mathematics, all systems of equations that express physical laws must be co-variant with respect to the Lorentz transformation. Thus, from the point of view of method, the special principle of relativity is comparable to Carnot's principle of the impossibility of perpetual motion of the second kind, for, like the latter, it supplies us with a general condition which all natural laws must satisfy.

Later, H. Minkowski found a particularly elegant and suggestive expression for this condition of co-variance, one which reveals a formal relationship between Euclidean geometry of three dimensions and the space-time continuum of physics.

Euclidean Geometry of Three Dimensions.	*Special Theory of Relativity.*
Corresponding to two neighbouring points in space, there exists a numerical measure (distance ds) which conforms to the equation $ds^2=dx_1^2+dx_2^2+dx_3^2$.	Corresponding to two neighbouring points in space-time (point events), there exists a numerical measure (distance ds) which conforms to the equation $ds^2=dx_1^2+dx_2^2+dx_3^2+dx_4^2$
It is independent of the system of co-ordinates chosen, and can be measured with the unit measuring-rod.	It is independent of the inertial system chosen, and can be measured with the unit measuring-rod and a standard clock. x_1, x_2, x_3 are here rectangular co-ordinates, whilst $x_4=\sqrt{-1}\,ct$ is the time multiplied by the imaginary unit and by he velocity of light.
The permissible transformations are of such a character that the expression for ds^2 is invariant, *i.e.* the linear orthogonal transformations are permissible.	The permissible transformations are of such a character that the expression for ds^2 is invariant, *i.e.* those linear orthogonal substitutions are permissible which maintain the semblance of reality of x_1, x_2, x_3, x_4. These substitutions are the Lorentz transformations.
With respect to these transformations, the laws of Euclidean geometry are invariant.	With respect to these transformations, the laws of physics are invariant.

From this it follows that, in respect of its *rôle* in the equations of physics, though not with regard to its physical significance, time is equivalent to the space co-ordinates (apart from the relations of reality). From this point of view, physics is, as it were, a Euclidean geometry of four dimensions, or, more correctly, a statics in a four-dimensional Euclidean continuum.

The development of the special theory of relativity consists of two main steps, namely, the adaptation of the space-time "metrics" to Maxwell's electro-dynamics, and an adaptation of the rest of physics to that altered space-time "metrics." The first of these processes yields the relativity of simultaneity, the influence of motion on measuring-rods and clocks, a modification of kinematics, and in particular a new theorem of addition of velocities. The second process supplies us with a modification of Newton's law of motion for large velocities, together with information of fundamental importance on the nature of inertial mass.

It was found that inertia is not a fundamental property of matter, nor, indeed, an irreducible magnitude, but a property of energy. If an amount of energy E be given to a body, the inertial mass of the body increases by an amount E/c^2, where c is the velocity of light *in vacuo*. On the other hand, a body of mass m is to be regarded as a store of energy of magnitude mc^2.

Furthermore, it was soon found impossible to link up the science of gravitation with the special theory of relativity in a natural manner. In this connection I was struck by the fact that the force of gravitation possesses a fundamental property, which distinguishes it from electro-magnetic forces. All bodies fall in a gravitational field with the same acceleration, or—what is only another formulation of the same fact—the gravitational and inertial masses of a body are numerically equal to each other. This numerical equality suggests identity in character. Can gravitation and inertia be identical? This question leads directly to the General Theory of Relativity. Is it not possible for me to regard the earth as free from rotation, if I conceive of the centrifugal force, which acts on all bodies at rest relatively to the earth, as being a "real" field of gravitation, or part of such a field? If this idea can be carried out, then we shall have proved in very truth the identity of gravitation and inertia. For the same property which is regarded as *inertia* from the point of view of a system not taking part in the rotation can be interpreted as *gravitation* when considered with respect to a system that shares the rotation. According to Newton, this interpretation is impossible, because by Newton's law the centrifugal field cannot be regarded as being produced by matter, and because in Newton's theory there is no place for a "real" field of the "Koriolis-field" type. But perhaps Newton's law of field could be replaced by another that fits in with the field which holds with respect to a "rotating" system of co-ordinates? My conviction of the identity of inertial and gravitational mass aroused within me the feeling of absolute confidence in the correctness of this interpretation. In this connection I gained encouragement from the following idea. We are familiar with the "apparent" fields which are valid rela-

tively to systems of co-ordinates possessing arbitrary motion with respect to an inertial system. With the aid of these special fields we should be able to study the law which is satisfied in general by gravitational fields. In this connection we shall have to take account of the fact that the ponderable masses will be the determining factor in producing the field, or, according to the fundamental result of the special theory of relativity, the energy density—a magnitude having the transformational character of a tensor.

On the other hand, considerations based on the metrical results of the special theory of relativity led to the result that Euclidean metrics can no longer be valid with respect to accelerated systems of co-ordinates. Although it retarded the progress of the theory several years, this enormous difficulty was mitigated by our knowledge that Euclidean metrics holds for small domains. As a consequence, the magnitude ds, which was physically defined in the special theory of relativity hitherto, retained its significance also in the general theory of relativity. But the co-ordinates themselves lost their direct significance, and degenerated simply into numbers with no physical meaning, the sole purpose of which was the numbering of the space-time points. Thus in the general theory of relativity the co-ordinates perform the same function as the Gaussian co-ordinates in the theory of surfaces. A necessary consequence of the preceding is that in such general co-ordinates the measurable magnitude ds must be capable of representation in the form

$$ds^2 = \sum_{uv} g_{uv} dx_u dx_v$$

where the symbols g_{uv} are functions of the space-time co-ordinates. From the above it also follows that the nature of the space-time variation of the factors g_{uv} determines, on one hand the space-time metrics, and on the other the gravitational field which governs the mechanical behaviour of material points.

The law of the gravitational field is determined mainly by the following conditions: First, it shall be valid for an arbitrary choice of the system of co-ordinates; secondly, it shall be determined by the energy tensor of matter; and thirdly, it shall contain no higher differential coefficients of the factors g_{uv} than the second, and must be linear in these. In this way a law was obtained which, although fundamentally different from Newton's law, corresponded so exactly to the latter in the deductions derivable from it that only very few criteria were to be found on which the theory could be decisively tested by experiment.

The following are some of the important questions which are awaiting solution at the present time. Are electrical and gravitational fields really so different in character that there is no formal unit to which they can be reduced? Do gravitational fields play a part in the constitution of matter, and is the continuum within the atomic nucleus to be regarded as appreciably non-Euclidean? A final question has reference to the cosmological problem. Is inertia to be traced to mutual action with distant masses? And connected with the latter: Is the spatial extent of the universe finite? It is here that my opinion differs from that of Eddington. With Mach, I feel that an affirmative answer is imperative, but for the time being nothing can be proved. Not until a dynamical investigation of the large systems of fixed stars has been performed from the point of view of the limits of validity of the Newtonian law of gravitation for immense regions of space will it perhaps be possible to obtain eventually an exact basis for the solution of this fascinating question.

Relativity: The Growth of an Idea.

By E. CUNNINGHAM.

SACCHERI, in his "Logica Demonstrativa," published in 1697, ten years after Newton's "Principia Mathematica," lays down a distinction between *real* and *nominal* definitions which should be kept in mind if we are to do justice to Newton. Euclid defines a square as a four-sided figure the sides of which are all equal, and the angles of which are all right-angles. That is what he means by the name "square." It is a *nominal* definition. It remains to be shown that such a figure exists. This is done in Book I., Prop. 46. The definition then becomes *real*. Euclid is not guilty of the error of presupposing the existence of the figure.

Newton prefixes to his principles of natural philosophy certain definitions of absolute, true, and mathematical space and time. The former remains fixed and immovable; the latter flows uniformly on, without regard to material bodies. He strives here against the imperfections of language to give words to the thought in the back of his mind. The philosopher attacks him on these definitions; he has no right to presuppose that these words correspond to any reality. What then? Suppose these offending definitions removed, or recognised as purely nominal. Then the definitions of velocity, acceleration, mass, and force are nominal, too, and the whole of Newton's structure of dynamics is a paper scheme of words and relations which may or may not correspond to the world of sense.

But that is exactly what it is. That is what all scientific theory is, until experiment demonstrates that the correspondence exists. The justification of Newton's theory comes, not in the discovery of a time that flows uniformly on, but in the fact that the observed phenomena of the tides, of planetary motion, and of mechanics in general do fit on to his scheme. But the fit does not consist

in the agreement of clock-time or solar time with absolute time. It is in the mutual agreement of the motions of clocks, earth, planets, moon, tides, and the rest; so that it matters not a whit whether the time variable used in the calculations flows uniformly on, or whether it is the merest variable number devoid of any physical significance whatever.

Thus the great paradox, the stumbling-block of absolute rotation, is not one of logic. Nature thrusts it upon us. While we would all admit that *a priori* rotation relative to empty space is a meaningless term, yet there remain Newton's bucket and Foucault's pendulum; and, strive as we will, Newton's way of looking at them remains the simplest. We would all fain think that "acceleration relative to empty space" is an empty phrase, and yet Nature is such that Newton's system of dynamics made modern science possible. Whatever we may say of Newton's definitions, therefore, his space and time are natural products of Nature's laboratory, to be purified, perhaps, but not to be rejected as spurious.

The nineteenth century was the æther age. From Arago, in 1820, to Michelson and Morley (1881–1905), followed by Trouton, Noble, Rankine, and Brace, vain attempts were made to discover the earth's velocity through the æther. But what, after all, was this elusive medium? To Faraday doubtless it was as real as anything in the universe. His resolute insistence on the need for a medium of transmission of electrical action, his discovery of the induction of currents by changing magnetic fields, Maxwell's advocacy of his views, and the demonstration of the electromagnetic nature of light, led up directly to the electron theory of Lorentz and Larmor, in which the æther appears as the background of all material phenomena. Yet the æther, after all, was but a name, and the electron theory a formal scheme of relations; like Newton's scheme, it was to be verified by its correspondence with actual observation. When it comes to observation, the only possibility is to note the behaviour of material bodies. Thus the æther with the associated ideas of electric and magnetic intensity ranks *pari passu* with Newton's absolute time and space. It has no likeness with matter; it is even doubtful whether it is of such substantiality that any element of it has identity. It has been said of Larmor's account of it that it reduces the universe to a set of differential equations, which, of course, is to a great extent true of Newton's work also.

But the notable thing about this theory is that it leaves the problem of absolute motion and absolute rotation exactly at the point where Newton left it. It gives a reasonable account of the failure of Michelson and Morley and the rest to discover the earth's velocity relative to the æther, just as Newton's theory would show why its velocity in absolute space cannot be determined. The æther-builders succeeded too well, and constructed, not one, but an infinity of æthers, any one having a uniform translatory motion relative to any other. But in the new theory, as in the old, a body may be said in an absolute sense to be devoid of rotational motion.

With the lack of determinateness in the æther goes a similar ambiguity in the measures of time and space. Each of these æthers has its proper scale of time and space. Events which are simultaneous in one æther are not simultaneous in another, and, since none can tell which is the true æther, none can tell whether two events are simultaneous or not. This is where the theories of Lorentz and Larmor lead us; and yet they only suggest it without demonstrating it. For neither of them professed to give a complete account of the structure of matter. The relations they propose for the æthereal action need to be supplemented by hypotheses as to the nature of the electron before they make a complete scheme; and when it comes to this Lorentz suggests a hypothesis, that of the contracting electron, which is devised specially for the purpose of getting the æther to conceal itself.

This brings us to the point at which Einstein makes his very modest suggestion (1905) of testing the hypothesis that in the nature of things we must not expect ever to find evidence of an absolute velocity of the earth through space. The ground had been well prepared. The electron theory as generally accepted was taken as it stood. The only new idea was that whereas his predecessors clung to the thought that one æther with its associated system of space-time measurement was real and the rest fictitious, Einstein, in the absence of any distinguishing features, ranks them all alike. To which some replied, "Give us back our absolute time and our æther." Yet Einstein had logic on his side, for the definitions of æther and of time are definitions *quid nominis*, not *quid rei*.

Looking back, we realise two great gaps in the special principle of relativity as Einstein propounded it in 1905. The first is the same which revealed itself in Newton's theory. "Uniform motion in a straight line" is left as a phrase with meaning, a situation as intolerable to the philosopher as the recognition of "absolute rest." Further, while it accepted the electromagnetic theory intact as a common basis of the structure of matter, it took no account of the other common property of matter, gravitation, of which so far the electromagnetic theory offered no explanation. Einstein at once realised the gaps, however, and saw a relation between them. Gravitational problems are problems of acceleration. The magnitude of the velocity of an observer does not affect his sense of gravity, but the experience of the man in the lift is that his acceleration does so. Thus Einstein perceived that to settle his difference with the philosopher he must first reconcile gravitation. Whether he would ever have done it without the genius of Minkowski we cannot tell. But as a matter of history the pure mathematician, who surely had learned much from Riemann and Helmholtz, was the first to emphasise the profound

unity between the categories of space and time that was suggested by Einstein's work. A universe of determinate relations, in which everything proceeds according to known laws, is one which may be surveyed as a great whole in which past, present, and future are one. It is a four-dimensional unity, and for Einstein any plane section through it could be the present. The history of a particle is a simple chain of points in that whole, a "world-line."

Now events moved fast. Minkowski gave the notion of a four-dimension universe. Riemann had initiated a method of geometry in any number of dimensions; Einstein had glimpsed the possibility that, taking gravitation into account, light might be subject to acceleration, which, being interpreted in Minkowski's fashion, meant that the ordinary Euclidean expression for the element of length in a four-dimension space must be generalised. This made the work of Riemann not only useful, but also prophetic. In 1853 he had written that "the ground of the measure relations existing in the universe, if continuous, must consist in the binding forces acting upon it." This really implies the whole relativity doctrine; it asserts that the measure relations of the phenomena perceived in the universe are incapable of determination on any absolute scale, independent of the phenomena themselves. Sixty years later Einstein perceives that the gravitational field must be included among those binding forces, and must affect profoundly the measure relations in every physical aspect. Having foreseen this, Riemann had proceeded to develop a non-Euclidean geometry in any number of dimensions, and thus the germ of the calculus that Einstein needed was created. With what success it was wielded is now well known.

Not so well known is the more recent work of Weyl. Einstein finds in universal gravitation the ground of the measure relations of the universe. But equally universal is the fact of electricity, and this universal "binding force" must equally take a part in those relations. The acuteness of Riemann's vision is doubly emphasised when we hear that Weyl discovers a further generalisation of his geometrical method which provides for electricity a place as natural and convincing as that taken by gravitation in Einstein's theory. But that is history yet in the making, and this article seeks only to traverse the course of history already complete.

Relativity and the Eclipse Observations of May, 1919.

By SIR FRANK DYSON, F.R.S.

IN 1915 Prof. Einstein predicted, as a consequence of the generalised theory of relativity, that a ray of light from a star would be bent in its passage through the sun's gravitational field. The amount of this deflection he gave as $1{\cdot}75''(a/r)$, where a is the sun's radius, and r the nearest distance of the ray to the sun's centre. As a ray of light reaches us in the direction of the tangent to its path, the apparent position of a star, photographed during an eclipse of the sun, should therefore be displaced by an angle $1{\cdot}75''(a/r)$ outwards from the sun's centre. The field of stars surrounding the sun should thus be distorted in a definite manner by an amount within the range of accurate astronomical observation. In Fig. 1, if a, b, c are the positions of the stars as seen at ordinary times, and if S is the centre of the sun during an eclipse, then the positions in which the stars are seen or photographed during an eclipse will be A, B, C. If a were at a distance 30′ from the sun's centre the displacement aA would be $0{\cdot}87''$, and if B were at a distance 90′ from the sun's centre its displacement would be $0{\cdot}27''$. These are, roughly speaking, the greatest and least displacements which can be obtained in practice. Nearer than 30′ from the sun's centre a star's image is liable to be drowned in the corona. At greater distances than 90′ good images can scarcely be obtained with a simple object glass, while a doublet or other combination introduces some difficulties.

These displacements, though small, are ten times as large as those met with in determinations of stellar parallax, and are determined in a precisely similar manner by comparison of photographs taken at different epochs. The field of stars photographed during the eclipse must be compared with the same field photographed when the sun is in another part of the sky. Experience shows that telescopes of as long focal length as practicable should be used, and that the eclipse field and comparison field should be photographed under as nearly as possible identical conditions as regards both the instruments themselves and the position of the field in the sky. With these precautions there was every reason to suppose that, given favourable weather, successful results would be obtained.

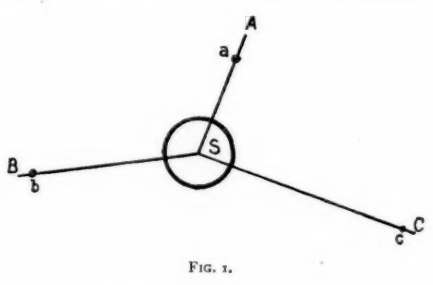

FIG. 1.

In the short time available for preparation it was not possible to arrange for the telescopes to be mounted equatorially, and the field of stars was reflected into fixed telescopes by cœlostat

mirrors. The possibility had to be faced that the mirrors might suffer some distortion from the sun's heat. Apart from this, two of the mirrors did to some extent spoil the sharpness of the stellar images.

Three series of photographs were taken. Prof. Eddington and Mr. Cottingham at Principe had very cloudy weather, but obtained photographs showing a few stars. The check field obtained on two nights shortly after the eclipse gave images similar to those on the plates taken during the eclipse. The temperature during the eclipse was 77° F., and 76·5° F. when the check fields were taken. There was no reason to suppose any change of scale, and on this assumption the plates when measured gave for the deflection at the sun's limb $1{\cdot}61'' \pm 0{\cdot}30''$.

Dr. Crommelin and Mr. Davidson in Brazil were favoured with ideal weather conditions. They found, however, that the images on the eclipse plate differed from those taken the previous night, and from those taken on the same field of stars two months later. This is attributed to the effect of the sun's heat in distorting the cœlostat mirror. If it is assumed that the scale has changed, then the Einstein deflection from the series of plates is 0·90″; if it is assumed that no real change of focus occurred, but merely a blurring of the images, the result is 1·56″; little weight is, however, attached to this series of photographs.

With a smaller lens of 4-in. aperture and 19-ft. focus the same observers were extremely successful. The images taken during the eclipse are in sharp focus and exactly similar to those on the comparison field. The result of measurement gave $1{\cdot}98'' \pm 0{\cdot}12''$ for the deflection at the limb; seven stars were photographed, and the individual displacements, both in right ascension and in declination, were in good accordance with the law $1{\cdot}75''(a/r)$. It has been pointed out by Prof. H. N. Russell that the photographs show a difference of scale of one part in 12,000 in the horizontal and vertical directions of the cœlostat mirror, and that if allowance be made for this the results for individual stars will be in still closer accordance.

The result of the eclipse, particularly of the second series of photographs at Sobral, is a close verification of Einstein's predicted displacements $1{\cdot}75''(a/r)$ radial from the sun's centre.

We proceed to consider the objections raised against this result. It has been suggested that allowance was not made correctly for the ordinary terrestrial refraction. The method adopted was that usually employed in astronomical photography, the second order terms being omitted, as they in no case amount to more than 0·02″. The possibility of distortion of the film of the photographs, owing to the presence of the corona, has also been suggested. Examination of the plate in Phil. Trans., vol. ccxx., will show how different the conditions are from those cases in which distortion of the film has been observed. The possible displacements on the eclipse photographs could not amount to 0·05″ for any of the stars, and would be in the wrong direction.

Prof. Anderson made the interesting suggestion that a possible error might arise from the fall of temperature in the air as the moon's shadow moves over the place of observation, thus causing an exceptional refraction. It has been shown by Prof. Eddington and Sir Arthur Schuster (NATURE, vol. civ., pp. 372, 468) that this effect would in all cases be very much smaller than the quantities in question. Further, it happens that at Sobral, owing to cloud in the early morning, which checked the normal daily rise of temperature, the fall during the eclipse was not more than 2° or 3° F.

The possibility that the observed displacements may be due to refraction by gaseous matter surrounding the sun has received a good deal of consideration. If such an atmosphere is controlled entirely by the sun's gravitation, an impossibly high density is required. Prof. Newall has investigated the consequences of assuming that gravitation is partly balanced near the sun by electrical forces and radiation pressure assumed to vary as the inverse square of the distance from the sun's centre. To obtain agreement with the eclipse observations, he finds it necessary that 0·999 of the weight of the gas should be thus balanced. The difficulties of such a supposition have been pointed out by Prof. Lindemann (*Observatory*, April, 1920). Further, Dr. Crommelin has directed attention to the fact that several comets have approached nearer the sun than paths of the rays of some of the stars photographed at the eclipse, yet their motion has not been retarded, or their substance entirely vaporised, although they were for two hours at this small distance from the sun and moving with a velocity of 300 kilometres a second.

Relativity and the Motion of Mercury's Perihelion.

By DR. A. C. D. CROMMELIN.

THE effect of Einstein's law in changing the position of Mercury's perihelion is sometimes looked on as something mysterious and recondite; but in reality anything that disturbs the law of inverse squares in the smallest degree is bound to produce such a shift; and as in all the systems known to us such disturbing factors exist, the line of apses is invariably in motion.

It is easy to show that when the central force falls off more rapidly than the ratio of inverse squares, the apse-line *advances*; for we may consider the inverse square law as holding, with the

addition of a small superposed force, outward in the outer half of the orbit, and inward in the inner half. These additional forces clearly cause the outward or inward motion to persist a little longer than it would otherwise do, thus delaying the attainment of the apses. It is proved in Tait and Steele's "Dynamics of a Particle," p. 127, that if the force vary as r^n, the apsidal angle in a nearly circular orbit is $\pi(3+n)^{-\frac{1}{2}}$. Put $n=-2(1+d)$, where d is very small, then the apsidal angle is $\pi(1-2d)^{-\frac{1}{2}}=\pi(1+d)$. Or, in a complete revolution, the apse advances $2\pi d$, which is a constant for all orbits.

This was the hypothesis advanced by Prof. Asaph Hall to explain the motion of Mercury's apses. The shift in the case of Mercury is 0·1″ per revolution. Applying this to the moon, it would give an apsidal advance of 135″ per century above the amount indicated by Newtonian theory. At the time Prof. Newcomb adopted the Hall formula, there appeared to be such an excess of motion of the lunar perigee; but further research, both on the side of theory (Prof. Brown) and on that of observation (Dr. Cowell and others), has emphatically proved that the actual excess is far smaller, and quite consistent with the 2″ per century resulting from Einstein's theory. Thus Brown found 14643527″ and 14643511″ for the theoretical centennial motion on two different assumptions of the earth's oblateness (Mon. Notices, vol. lxiv., p. 532). His discussion gave as the observed value 14643523″, while Cowell found 14643538″. In any case, the difference between theory and observation is very much smaller than that required by Hall's law. The latter is thus definitely put out of court, and it becomes a matter of regret that Newcomb adopted it in his tables of the four inner planets. It meant a more drastic alteration of the Newtonian law than that effected by Einstein; the former alters the law in all circumstances, while the latter leaves it unaltered for bodies at rest, but introduces a term that increases the force where there is relative motion.

The second suggestion in explanation of the motion of Mercury's apse is oblateness of the sun. It is easy to show that the attraction of an oblate body falls off more rapidly than the inverse square, thus producing advance of the apse of a satellite. Most of the satellites of the solar system afford examples; the most striking case is Jupiter V., the apse of which makes two entire revolutions in a terrestrial year. Where the satellite does not revolve in the equatorial plane of its primary, there is a second effect of oblateness; it causes the satellite's orbit-plane to shift, its pole describing a circle round the planet's pole. We may refer, for example, to the system of Mars. H. Struve determined the position of the pole of Mars and the amount of oblateness from observations of its satellites; similarly the position of Neptune's pole can be approximately inferred from the change in the orbit-plane of its satellite.

The amount of oblateness of the sun necessary to account for the motion of Mercury's perihelion is not great. Newcomb deduced that the necessary excess of the equatorial diameter over the polar would be slightly more than ½″; the existence of even this small excess is rendered highly improbable by the very numerous measures of the solar disc, notably the heliometer measures in connection with the Venus-transits of 1874 and 1882; these were discussed by Dr. Auwers, and seemed, if anything, to indicate that the polar diameter is the greater. A further objection is that the solar equator is inclined 3° 21′ to Mercury's orbit, and its oblateness would produce a diminution of 2·6″ per century in the inclination of Mercury's orbit. Observation, if anything, indicates a shift of the inclination in the opposite direction, and the amount 2·6″ is so far beyond the probable error as to render the theory of solar oblateness untenable. The above points were established by Newcomb, "Elements of the Four Inner Planets," in 1895, so that it is strange to find this untenable hypothesis still freely suggested in the United States.

It is fairly obvious that the portion of the zodiacal light that is inside Mercury's orbit would produce effects of the same general kind as those arising from solar oblateness. Now observations of the zodiacal light indicate a smaller inclination to the ecliptic than 7°, the inclination of Mercury's orbit. Thus J. F. J. Schmidt found values ranging from 4° to 0°, and Prof. Douglas's photograph, taken at Flagstaff, Arizona, on March 19, 1901, shows the light almost symmetrical on each side of the ecliptic. Now, unless the mean plane of the light agreed with that of Mercury's orbit, its gravitational effect on the apse of that planet would be accompanied by a shift of its orbital plane, not verified by observation. It has been found impossible to assign any position to hypothetical perturbing matter that would explain the apse motion of Mercury without causing other anomalies in the elements of that planet and of Venus, which are negatived by observation. Moreover, there is the difficulty that the zodiacal light, if of sufficient mass to produce such an effect, should give us more light than it does. Dr. H. Jeffreys examines the question in Mon. Not., vol. lxxx., p. 138. He shows that if the light arises from reflection by the molecules of a gas, the effect on Mercury would be only 1/3000 of that observed. If the light arises from reflection by solid particles, he takes 10 km. as their maximum admissible diameter (probably far in excess of what is really tenable); he still finds that their gravitational effect would be only 1/20 of that required. He makes similar calculations for the corona, reaching like conclusions.

Hence we seem to be driven by exhaustion to the Einstein law as the only satisfactory explanation. It clearly can have no effect on orbital planes, so it produces accordance in apsidal motion without introducing other anomalies. Further, it was not an *ad hoc* hypothesis; it was reached on independent grounds, and it was an undesigned coincidence that it fitted so well for

Mercury's apsidal motion. For the expression $3\left(\frac{\text{velocity of planet}}{\text{velocity of light}}\right)^2$, which is the angular motion of the apse (in fractions of a circumference) per revolution of the planet, involves no empirical or arbitrary constant. We can express the reason for the advance in simple terms thus: At infinity the relative velocity is zero, and the law is the Newtonian one, but the nearer we approach the central orb the higher becomes the velocity, and the greater the extra force. Hence we have another case of the force falling off more rapidly than the inverse square, which we have seen to lead to apsidal advance.

It is interesting to note that the advance per revolution varies as (velocity)^2 or as $1/a$. Hence the advance per century varies as $a^{-\frac{5}{2}}$, or it falls off much more rapidly with increase of a than the Hall law, which gives $a^{-\frac{3}{2}}$. In the course of centuries this would discriminate between them, independently of the lunar test; but the orbits of Venus and the earth are so nearly circular that the time for that test has not yet arrived.

In the case of Mars we may note that F. E. Ross's rediscussion of the observations of that planet and of the mass of Venus takes off some 2″ from Newcomb's value of its excess of apsidal motion in a century. When we further remove the Einstein term 1·3″, we are left with some 2·7″; as the actually observed quantity is the product of 2·7″ by the eccentricity (1/11), it falls well within the limits of observational error.

The Displacement of Solar Lines.

By DR. CHARLES E. ST. JOHN, Mount Wilson Solar Observatory, Pasadena, California.

THE agreement of the observed advance of Mercury's perihelion and of the eclipse results of the British expeditions of 1919 with the deductions from the Einstein law of gravitation gives an increased importance to observations on the displacement of absorption lines in the solar spectrum relative to terrestrial sources, as the evidence on this deduction from the Einstein theory is at present contradictory. Particular interest, moreover, attaches to such observations, inasmuch as the mathematical physicists are not in agreement as to the validity of this deduction, and solar observations must eventually furnish the criterion.

Prof. Eddington's view, if I understand it, is that the theory cannot claim support from the present evidence, nor can observed displacements not agreeing with the theory be on that account regarded as in the slightest degree adverse to it, the only possible conclusion in his view being that there are certain causes of displacement of the lines acting in the solar atmosphere and not yet identified ("Space, Time, and Gravitation," p. 130).

The great majority of metallic lines observed for differences between their wave-lengths in the sun and terrestrial sources do show displacements. These differ in most cases from those deduced from the Einstein law of gravitation in ways as yet unexplained. If reasonable solar causes can be adduced to account for the wide discrepancies between theory and observation, the position of the generalised theory of relativity would be greatly strengthened.

According to the theory, the displacements are to the red, and are proportional to wave-length, being independent of the intensity of the lines and of the element to which they belong. Observational results differ from those deduced from the theory in at least four important ways. The observed displacements are not proportional to the wave-length; they differ from element to element in the same spectral region; for the same element and spectral region they vary with line-intensity; the displacements show both large positive and negative divergences from the calculated values. Interesting examples are found in Jewell's early work (*Astrophysical Journal*, vol. iii., p. 89, 1896). The relative values here are of high weight, and the data are important in that the range of elements is wider than occurs in more recent investigations. Divergences in the four directions from the calculated displacements are shown in the following extract from his observations on the differences in wave-length between 115 solar and arc lines:—

			Mean λ	Δ λ Obs.	Δ λ Cal.	Obs. – Cal.
Calcium	H,K,	4227	4042	+0·019	+0·009	+0·010Å
Calcium	Int.	1 – 6	5227	+0·004	+0·011	−0.007
Iron	,,	10–40	3950	+0·008	+0·008	±0·000
Iron	,,	2– 8	3950	+0·003	+0·008	−0·005
Aluminium	,,	15–20	3950	+0·006	+0·008	−0·002
Nickel	,,	10–15	3530	+0·017	+0·008	+0·009
Nickel	,,	1– 5	3625	+0·005	+0·008	−0·003
Copper	,,	6– 9	3262	+0·006	+0·007	+0·009
Potassium	,,	00– 0	4046	−0·008	+0·009	−0·017

For statistical discussion the quantity of data available is as yet quite inadequate even in the case of iron, the most widely studied element. Not only should the terrestrial and solar wave-lengths be known to high precision over the widest possible range of spectrum, but also the pressure shift per atmosphere. Unfortunately, there are no published data on the wave-lengths and pressure displacements of the iron lines, in which, over a long spectral range, the errors due to pole-effect in the arc are reduced to the magnitude of the calculated Einstein displacement. For other metallic elements the data are even more deficient. With a sufficiently large and varied accumulation of material there is hope that the complex solar conditions may be analysed, and the contributions to the observed effects arising from the various causes determined with some certainty. The pressing need is for data of the requisite accuracy and variety. This need adds interest to determinations of wave-lengths and of pressure dis-

placements, and to investigations of the characteristic behaviour of spectrum lines, as all such data will have a part in solving one of the most absorbing questions in cosmic physics.

Evershed adduces his observations upon the spectrum of Venus as evidence of an "earth-effect" driving the gases from the earth-facing hemisphere of the sun, and he would by this hypothetical action explain the observed displacements of the solar lines, and thus negative the deduction from the Einstein theory. Two series of Venus observations have been made by Dr. S. B. Nicholson and myself. The details will appear in a forthcoming Contribution from the Mount Wilson Observatory. Our observations indicate that the displacements of the Venus lines to the violet relative to skylight are due to non-uniform illumination of the slit when the guiding is done upon the visual image, the effect increasing with the refraction and becoming more evident the smaller the image. The explanation is based upon the observation that spectrograms taken at low altitudes give larger displacements to the violet than those taken on the same night at higher altitudes, and that the displacements correlate with the cotangent of the altitude and the reciprocal of the diameter of the planet at the time of observation.

In respect to the observations at Mount Wilson on the lines of the cyanogen band at λ3883, I have as yet found no grounds for considering them seriously in error. The explanation of the results adverse to the theory based upon dissymmetry appears inadequate (*Observatory*, p. 260, July, 1920), and the assumption that the adverse results are due to superposed metallic lines seems to be negatived by the observations of Adams, Grebe, Bachem, and myself that for these lines there is no displacement between the centre and limb of the sun. Metallic lines as a class shift to the red in passing from the centre to the limb. If, then, metallic lines are superposed on these band lines in such a way as to mask the gravitational displacement to the red when observed at the centre of the sun, this should be revealed by a shift to the red at the limb.

The lines of the cyanogen bands are under investigation in the observatory laboratory both as reversed in the furnace and as produced in the arc under varying pressure. The measures show no evidence of a displacement to the red under decreased pressure as indicated by Perot's observations.

The present programme at Mount Wilson aims at an accumulation of varied and extensive data that will furnish a suitable basis from which to approach the general question of the behaviour of Fraunhofer lines relative to terrestrial sources.

Non-Euclidean Geometries.

By PROF. G. B. MATHEWS, F.R.S.

THE ordinary theory of analytical geometry may be extended by analogy as follows: Let $x_1, x_2, \ldots x_n$ be independent variables, each ranging over the complete real (or ordinary complex) continuum. Any particular set $(x_1, x_2, \ldots x_n)$, in that order, is said to be a point, the co-ordinates of which are these x_i; and the aggregate of these points is said to form a point-space of n dimensions (P_n). Taking $r<n$, a set of r equations $\phi_1=0, \phi_2=0, \ldots \phi_r=0$, connecting the co-ordinates, will in general define a space P_{n-r} contained in P_n. Theorems about loci, contact, envelopes, and the principle of duality all hold good for this enlarged domain, and we also have a system of projective geometry analogous to the ordinary one.

Physicists are predominantly interested in metrical geometry. The ordinary metrical formulæ for a P_3 may be extended by analogy to a P_n; there is no logical difficulty, but there is, of course, the psychological fact that our experience (so far) does not enable us to "visualise" a set of rectangular axes for a P_n if $n>3$; not, at least, in any way obviously analogous to the cases $n=2, 3$.

In ordinary geometry, for a P_3 we have the formula

$$ds^2=dx_1^2+dx_2^2+dx_3^2$$

for the linear element called the distance between two points (x), $(x+dx)$. Riemann asked himself the question whether, for every P_n, this was necessarily a typical formula for ds, on the assumption that solid bodies can be moved about in space without distortion of any kind. His result is that we may take as the typical form, referred to orthogonal axes,

$$ds^2=\Sigma dx^2/N^2,$$

where

$$N=1+\tfrac{1}{4}\alpha\Sigma x^2,$$

and α is an arbitrary constant, called the *curvature* of the P_n in question. This curvature is an intrinsic property of the P_n, and should not be considered as a warp or strain of any kind. When $\alpha=0$, we have the Euclidean case. As an illustration of the theory that can be actually realised, take the sphere $x^2+y^2+z^2=r^2$ in the ordinary Euclidean P_3. By putting

$$D=u^2+v^2+4r^2,$$
$$Dx,\ Dy,\ Dz=4r^2u,\ 4r^2v,\ (u^2+v^2-4r^2)r,$$

the equation $x^2+y^2+z^2=r^2$ becomes an identity, and we may regard the surface of the sphere as a P_2 with (u, v) as co-ordinates. The reader will easily verify that

$$ds^2=(du^2+dv^2)\div\{1+\frac{1}{4r^2}(u^2+v^2)^2;$$

so we have a case of Riemann's formula with $\alpha=r^{-2}$. We *cannot* find a similar formula for the surface of an ellipsoid, because a lamina that "fits" a certain part of the ellipsoid cannot be

freely moved about so as to remain in contact with the surface.

To avoid misunderstanding, it should be said that Riemann's expression for ds^2 is not the only one that is taken to be the typical or standard formula. The important thing is that, given any formula for ds^2, in a P_n, we can, by direct calculation, find an expression for the curvature of P_n in the neighbourhood of any assigned point (x). It is only when this curvature is everywhere the same that we have a P_n for which the axiom of free mobility is valid. When the curvature varies from place to place we are not entitled, for instance, to assume that we can carry about an invariable foot-rule for purposes of physical measurement.

In the simpler theory of relativity we have a formula

$$ds^2=dx^2+dy^2+dz^2-c^2dt^2, \quad \ldots \quad (1)$$

where c is a real constant. As it originally presents itself, x, y, z are ordinary rectangular coordinates, t is the time, and c the experimental velocity of light. By a suitable choice of units we can make the value of c any finite constant that we please. Following Minkowski, I shall call (x, y, z, t) a world-point; the aggregate of these points may be provisionally called a space-time world $P(x, y, z, t)$.

When $t=t_0$, a constant, $dt=0$ and (1) reduces to the ordinary Euclidean formula. We may express this by saying that the sub-world $P(x, y, z, t_0)$ is Euclidean. Actual experiments take time; so we cannot verify this assertion by observation. If, however, two observers, at different places, make measurements which begin and end at the same instants, we may expect their results to be consistent. As Prof. Einstein has pointed out, the question of simultaneity (and, indeed, of time itself, as an *observed* quantity) is a more difficult one than appears at first sight.

The main difficulty about (1), as it seems to me, is that the expression on the right is not a definite form; hence in the neighbourhood of every "real" point (x, y, z, t) there is a real region for which ds^2 is negative. It is possible that the difficulty of interpretation is more apparent than real, as is the case in some well-known examples. For instance, a hyperbola may be analytically defined as an ellipse of semiaxes a, bi, where a, b are real; and, *moreover*, v. Staudt's theory of involution gives an actual geometrical meaning to the algebraic definition.

If, with $i^2=-1$, we put $ct=i\tau$, the formula (1) becomes

$$ds^2=dx^2+dy^2+dz^2+d\tau^2, \quad \ldots \quad (2)$$

the typical formula for a Euclidean P_4. This makes it very tempting to assume that the successions of phenomena in our world of experience are, so to speak, sections of a *space-world* $P(x, y, z, \tau)$, obtained by giving τ purely imaginary values. This point of view has been taken by Minkowski and others.

The mathematical theories of abstract geometry and kinematics are so complete that physicists have a definite set of hypotheses from which to choose the one most suited to their purpose; and besides this they have to frame axioms and definitions about time, energy, etc., with which the pure mathematician is not concerned.

Whatever may be the ultimate form given to the theory of relativity, the predictive quality of its formulæ gives it a high claim to attention, and it certainly seems probable that, for the sake of what Mach calls economy of thought, we may feel compelled to change our ideas of "actual" space and time.

In an article like this it is impossible to go into detail; the following references may be useful to readers who desire further information:—"The Elements of Non-Euclidean Geometry," by J. L. Coolidge, is rather condensed, but very conscientious and trustworthy; one of the best analytical discussions of the metrical theory is in Bianchi's "Lezioni di Geometria Differenziale," chap. xi.; and Lie's "Theorie der Transformationsgruppen," vol. iii., chaps. xx.–xxiv., contains a most valuable critique of Riemann and Helmholtz. The article "Geometry" in the "Encyclopædia Britannica" (last edition) gives an outline of the theory and numerous references. Finally, there is an elaborate "Bibliography of Non-Euclidean Geometry" by D. M. J. Somerville (see NATURE, May 16, 1912, vol. lxxxix., p. 266).

The General Physical Theory of Relativity.

By J. H. JEANS, Sec. R.S.

THE relativity theory of gravitation, which is at present the centre of so much interest, owes its existence to an earlier physical theory of relativity which had proved to be in accord with all the known phenomena of Nature except gravitation. The gravitational theory is only one branch, although a vigorous and striking branch, of a firmly established parent tree. The present article will deal solely with the main trunk and roots of this tree.

Newton's laws of motion referred explicitly to a state of rest, but also showed that the phenomena to be expected from bodies in a state of rest were precisely identical with those to be expected when the same bodies were moving with constant velocity. Indeed, Newton directed special attention to this implication of his laws of motion in the following words:—

COROLLARY V.: *The motions of bodies included in a given space are the same among themselves, whether that space is at rest, or moves uniformly forwards in a right line without any circular motion.*

"A clear proof of which we have," continues Newton, "from the experiment of a ship, where all motions happen after the same manner whether

the ship is at rest, or is carried uniformly forward in a right line."

Thus no experiment on board ship can ever disclose the ship's velocity through the sea. The matter stands differently to one who is free to experiment with both the ship and the sea. Let a sailor walk to the end of the bowsprit and drop his lead into the sea. A circular ripple will spread out; but every sailor knows that the point at which his line enters the water will not remain at the centre of this circle. The velocity with which the point of entry advances from the centre of the circle will give the velocity of the ship through the sea.

If our earth is ploughing its way through a sea of æther, an experiment conceived on similar lines ought to reveal the velocity of the earth through the æther. The famous Michelson-Morley experiment was designed to this end. Our earth was the ship; the physical laboratory at Chicago was the bowsprit. The dropping of the lead into the sea was represented by the emission of a light-signal, and the wave-front emanating from this signal was the ripple on the sea of æther. In the original experiments of Michelson and Morley it was not possible to watch the progress of the ripple directly, but sufficient information was obtained by arranging mirrors to reflect the signal back to the starting-point. In the recent experiments of Majorana this difficulty is obviated, although at the cost of some loss of refinement.

From these and other experiments the result invariably emerges that the wave-front appears to be a sphere having the observer at its centre. Thus on the hypothesis that our earth is surrounded by a sea of æther, experiment shows that the velocity of the earth relative to this sea of æther is *nil*. We cannot suppose that the true velocity is always *nil*, for the earth is known to be describing circles around the sun at a speed of 30 km. a second, while the experiments were sensitive enough to detect a velocity of one-hundredth part of this.

In view of the complete success which has attended the hypothesis of relativity, it would scarcely seem to be necessary to do more than mention the various early hypotheses put forward to account for these and similar experimental results. Such were the hypotheses that the earth drags the æther along with it (Arago, 1818); that matter moving through the æther is contracted, as a result of its motion, in just such a way as eternally to conceal the earth's motion through the æther from our measurements (FitzGerald, 1893; Lorentz, 1895); and that light is a phenomenon of corpuscular emission (Ritz). Each of these hypotheses explained some only of the facts to be explained, and failed with others.

The theory of light has progressed largely through the construction of mechanical models. Every such model, if fruitful, suggests new laws to be tested. So long as the laws suggested in this way are confirmed by observation, the model stands; as soon as a predicted law is found to fail, the model must be amended or abandoned. Notable examples of such models have been the corpuscular model of Newton, the elastic-solid æther of Young and his followers, and the electromagnetic æther of Faraday and Maxwell. The first two of these have long ago served their purpose and passed away. The time has now clearly come when the last of these optical models, the electromagnetic æther, must be either amended or abandoned, and the indications are strong that the less drastic course will not suffice.

The construction of mechanical models is not, however, the only known means of guidance to the discovery of new laws of Nature. An even more fruitful means of progress has been provided by tentative generalisation of known laws. Proved laws *a, b, c, d* . . . are found to be special cases of a more general law A, and the truth of A is then seen to involve not only the detailed laws *a, b, c, d* . . . which have led to A, but also other detailed laws *p, q, r, s* . . . In this way we may be guided to test the suggested new laws *p, q, r, s* . . . , and the generalisation A is, of course, strengthened or discredited according as *p, q, r, s* . . . are confirmed by observation or not. Conspicuous instances of successful generalisations of this kind are provided by the conservation of energy and the second law of thermodynamics.

Early in the present century Einstein and Lorentz suggested a tentative generalisation of this type, which is now known as the hypothesis of relativity. Since all experiments so far performed had failed to disclose the velocity of the earth through the assumed æther, it was natural to generalise in the first place to the tentative principle that this velocity could not, from the nature of things, ever be revealed by any experiment whatever. Generalised somewhat further by the removal of the local reference to our earth, the hypothesis assumed the form that all phenomena of Nature are the same for an observer moving with any uniform velocity as they are for an observer at rest. This somewhat crude form of statement shows that the hypothesis merely generalises Newton's corollary V. quoted above, so as to make it apply to all the phenomena of Nature. Since, however, the acceptance of the hypothesis makes it impossible to define what is meant by a state of rest, it is better to express the hypothesis in the form that all the phenomena of Nature are the same for any two observers who move relative to one another with a constant velocity.

This hypothesis is known already to be true as regards the mechanical forces considered in Newton's laws. Naturally, also, it is true as regards the optical phenomena investigated in the Michelson-Morley and similar experiments, for it is out of these phenomena that the hypothesis arose. The crucial test occurs when laws in other fields of science are deduced from the hypothesis and compared with observation. The hypothesis has been very thoroughly tested in the field of electromagnetism, and in every single case has emerged triumphant. As conspicuous instances of its success may be mentioned: The explanation

of Fizeau's water-tube experiment, the prediction of the law connecting electronic mass with velocity, and the prediction of ponderomotive electromagnetic forces in moving media.

One final, and therefore crucial, test remains: gravitation. It was soon noticed that the hypothesis was inconsistent with the exact truth of Newton's gravitational law of force mm'/r^2. Thus the hypothesis of relativity predicts that a freely moving planet cannot describe a perfect ellipse about the sun as focus. This prediction is made on quite general grounds, just as the conservation of energy predicts that a stream of water cannot flow uphill. But the conservation of energy by itself is powerless to predict what will be the actual course of a stream of water, and in precisely the same way the hypothesis of relativity alone is powerless to predict what will be the orbit of a planet. Before this or any other positive gravitational predictions can be made, additional hypotheses must be introduced. The main trunk of the tree is the relativity hypothesis already mentioned; these additional hypotheses form the branches. The trunk can exist without its branches, but not the branches without the trunk. Whether the branches have been placed on the trunk with complete accuracy is admittedly still an open question—it must of necessity remain so until the difficult questions associated with the gravitational shift of spectral lines have been finally settled—but the main trunk of the tree can be disturbed by nothing short of a direct experimental determination of the absolute velocity of the earth, and the only means which can possibly remain available for such a determination now are gravitational.

The Michelson-Morley Experiment and the Dimensions of Moving Bodies.

By PROF. H. A. LORENTZ, For.Mem.R.S.

AS doubts have sometimes been expressed concerning the interpretation of Prof. Michelson's celebrated experiment, some remarks on the subject will perhaps not be out of place here. I shall try to show, by what seems to me an unimpeachable mode of reasoning, that, if we adopt Fresnel's theory of a stationary æther, supposing also that a material system can have a uniform translation with constant velocity v without changing its dimensions, we must surely expect the result that was predicted by Maxwell.

Let us introduce a system of rectangular axes of co-ordinates fixed to the material system, the axis of x being in the direction of the motion. Then, with respect to these axes, the æther will flow with the velocity $-v$. The progress of waves of light, relatively to them, may be traced by means of Huygens's principle; for this purpose it suffices to know the form and position of the elementary waves. For the sake of generality I shall suppose the propagation to take place in a material medium of refractive index μ, so that, if c is the velocity of light in the æther, the velocity in the medium when at rest would be c/μ. The elementary wave formed in the time dt around a point P will be a sphere of radius $(c/\mu)dt$, of which the centre P′ does not, however, coincide with P, the line PP′ being in the direction opposite to that of OX, and having the length $(v/\mu^2)dt$ (Fresnel's coefficient).

If Q is any point on the surface of the sphere, PQ can be considered as an element of a ray of light, and $w = PQ/dt$ will be the velocity of the ray. Confining ourselves to terms of the second order, *i.e.* of the order v^2/c^2, and denoting by δ the angle between the ray and OX, we have

$$\frac{1}{w} = \frac{\mu}{c} + \frac{v}{c^2}\cos\delta + \frac{v^2}{2\mu c^2}(1+\cos^2\delta) \quad . \quad . \quad (1)$$

Now, let A and B be points having fixed positions in the material system. The course s of a ray of light passing from A to B will be determined by the condition that the integral

$$\int \frac{ds}{w} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

is a minimum. Using the above value of $1/w$, it is easily shown that, if quantities of the second order are neglected, the course of the ray is not affected by the translation v, so that, if L_0 is the path of the ray in the case $v=0$, and L the real path, these lines will be distant from each other to an amount of the second order only. Hence, if in the case of a translation v we calculate by means of (1) the integral (2), both for L and L_0, the two values will differ by no more than a quantity of the fourth order; indeed, since the integral is a minimum for L, the difference must be of the second order with respect to the distances between L and L_0, and these distances are already of the second order of magnitude.

It is seen in this way that, so long as we neglect terms of an order higher than the second, we may replace

$$\int_{L} \frac{ds}{w} \text{ by } \int_{L_0} \frac{ds}{w},$$

an argument that must not be overlooked in the theory of the experiment. On the ground of it we shall commit no error if, in determining the paths L_1 and L_2 of two rays that start from a point A, and are made to interfere at a point B, we take no account of the motion of the apparatus. The change in the difference of phase produced by the translation will be given by the difference between the values which the integral

$$\int \frac{v^2}{2\mu c^3}(1+\cos^2\delta)\,ds$$

takes for the lines L_1 and L_2 so determined. If, along the first of them, $\cos^2\delta = 1$, and along the

second $\cos^2\delta=0$, and $\mu=1$, the change will be the same as would be produced by a lengthening of L_1 in the ratio of 1 to $1+v^2/2c^2$. As no displacement of the fringes has been observed, we are led to the well-known hypothesis of a contraction of moving bodies in the direction of translation, in the ratio of 1 to $1-v^2/2c^2$.

We could now try to extend the above considerations to cases in which v/c, though always below 1, is no longer a small fraction. This would require somewhat lengthy calculations, into which, however, we need not enter here, because we know by the theory of relativity that the true value of the coefficient of contraction is $\sqrt{1-v^2/2c^2}$. I may remark here that there can be no question about the reality of this change of length. Suppose that, in studying the phenomena, we use a system of rectangular co-ordinates x_1, x_2, x_3, and a time t, and that in this system the velocity of light is c in all directions. Further, let there be two rods, I. and II., exactly equal to each other, and both placed in the direction of x_1, I. at rest in the system of co-ordinates, and II. moving in the direction of its length with a velocity v. Then, certainly, if the length of a rod is measured by the differences of the values which the co-ordinate x_1 has at the two ends at one and the same instant t, II. will be shorter than I., just as it would be if it were kept at a lower temperature. I need scarcely add that if, by the ordinary transformation of the theory of relativity, we pass to new co-ordinates x_1', x_2', x_3', t' in such a manner that in this system the rod II. is at rest, and if now we measure the lengths by the difference between the values of x_1' which correspond to a definite value of t', I. will be found to be the shorter of the two.

The question arises as to how far the dimensions of a solid body will be changed when its parts have unequal velocities, when, for example, it has a rotation about a fixed axis. It is clear that in such a case the different parts of the body will, by their interaction, hinder each other in their tendency to contract to the amount determined by $\sqrt{1-v^2/c^2}$. The problem can be solved by the ordinary theory of elasticity, provided only that this theory be first adapted to the principle of relativity. Indeed, we can still use Hamilton's principle:—

$$\delta\int_{t_1}^{t_2}dt\int(T-U)dS=0 \quad . \quad . \quad . \quad . \quad (3)$$

(dS, element of volume; T, kinetic, and U, potential, energy, both per unit of volume), if, by some slight modifications, the integral is made to be independent of the particular choice of co-ordinates. That this can be done, even in the general theory of relativity (theory of gravitation), is due to the possibility of expressing the length of a line-element in the four-dimensional space x_1, x_2, x_3, x_4 ($x_4=t$) in "natural units"—*i.e.* in such a manner that the number obtained for it is the same whatever be the co-ordinates chosen—and of measuring angles in a similar way. As is well known, the length ds of a line-element is given by the formula:—

$$ds^2=\Sigma(ab)g_{ab}dx_a dx_b \quad . \quad . \quad . \quad . \quad (4)$$

where the ten quantities g_{ab} ($g_{ab}=g_{ba}$) are the gravitation potentials, and the angle δ between two elements is determined by

$$\cos\delta\, ds\, d's=\Sigma(ab)g_{ab}dx_a d'x_b \quad . \quad . \quad . \quad (5)$$

In the sums, each of the indices a and b is to be given the values 1, 2, 3, 4. When the value 4 is excluded, as will be the case in some of the following formulæ, a Greek letter will be used for the index.

We can also find an invariant value l for the distance between two material particles P and P′ infinitely near each other. To this effect we have to consider the word-lines L and L′ of these particles in the space x_1, x_2, x_3, x_4. Let Q be the point of L corresponding to the chosen time x_4, and Q′ a point of L′ such that QQ′ is at right angles to L. Then the length of QQ′, determined by means of (4), will be the value required. Similarly, if P″ is a third particle, infinitely near P and P′, and Q″ the point of its word-line so situated that QQ″ is perpendicular to L, the angle P′PP″ will be taken to be the angle between the elements QQ′ and QQ″ determined according to (5).

As to the co-ordinates x_1, x_2, x_3, x_4, it may be recalled that, in a field free from gravitation, they may be chosen in such a manner (x_1, x_2, x_3 being at right angles to each other) that the velocity of light has the constant magnitude c; the potentials g_{ab} will in this case have the values

$$g_{11}=g_{22}=g_{33}=-1,\ g_{44}=c^2,\ g_{ab}=0 \text{ for } a\neq b.$$

These may be called the normal values of the potentials, and a system of co-ordinates for which they hold a normal system.

Let us now consider a solid body M, and let us first conceive it to be placed in a normal system of co-ordinates (S_0), and to be at rest in that system, free from all external forces. The body may then be said to be in its natural state, and its particles may be distinguished from each other by their co-ordinates ξ, η, ζ with respect to three rectangular axes fixed in the body. In all that follows, these quantities will be constant, and so will be the mass $\rho d\xi d\eta d\zeta$ of an element, ρ being the density in the natural state.

We shall now suppose the body to be placed in a system of co-ordinates x_1, x_2, x_3, x_4 (S), not necessarily normal, and to have some kind of motion in that system. It is this motion, in which x_1, x_2, x_3 will be definite functions of ξ, η, ζ, and x_4, which we want to determine by means of Hamilton's principle properly modified.

In order to get the new U, I shall introduce the dilatations ξ_ξ, η_η, ζ, and shearing strains ξ_η, η_ζ, ζ_ξ, with respect to the axes ξ, η, ζ. These quantities are defined as follows:—

Let P, P′ be the particles ξ, η, ζ, and $\xi+d\xi$, η, ζ and let l be their distance in the state considered

(system S), and l_0 their distance in the natural state (system S_0), these distances being both determined in the manner specified in what precedes. Then

$$\xi\xi=(l-l_0)/l_0.$$

Again, if P″ is the particle ξ, $\eta+d\eta$, ζ, and if the angle P′PP″, calculated as stated before, has in the two cases the values δ and $\delta_0(=\frac{1}{2}\pi)$, we shall have

$$\xi\eta=\delta_0-\delta.$$

The six deformations ξ_1 . . . will be considered as infinitely small. In the problem we have in view, they are of the order of magnitude v^2/c^2, so that our final result will be correct to that order.

If we put

$$U=A(\xi\xi^2+\eta\eta^2+\zeta\zeta^2)+\tfrac{1}{2}B(\xi\xi+\eta\eta+\zeta\zeta)^2+\tfrac{1}{2}A(\xi\eta^2+\eta\zeta^2+\zeta\xi^2), \quad . \quad (6)$$

the well-known expression for the potential energy of an isotropic elastic body, U will be invariant for any change of co-ordinates.

As to the kinetic energy T, it is to be replaced by an expression containing $\rho\frac{ds}{dt}$. Finally, we must write, instead of (3),

$$\delta\int_{t_1}^{t_2}\int(-c\rho-\frac{1}{c}U)\frac{ds}{dt}dtd\xi d\eta d\zeta=0.$$

We have still to add the formulæ that are found by working out the above definitions of ξ_ξ, ξ_η, etc., viz. :—

$$\xi_\xi=-\tfrac{1}{2}\Sigma(a\beta)g_{a\beta}\frac{\partial x_a}{\partial\xi}\frac{\partial x_\beta}{\partial\xi}+\frac{\left[\Sigma(a\beta)g_{a\beta}v_\beta\frac{\partial x_a}{\partial\xi}\right]^2}{2\Sigma(ab)g_{ab}v_av_b}-\tfrac{1}{2},$$

$$\xi_\eta=-\Sigma(a\beta)g_{a\beta}\frac{\partial x_a}{\partial\xi}\frac{\partial x_\beta}{\partial\eta}+\frac{\Sigma(a\beta)g_{a\beta}v_\beta\frac{\partial x_a}{\partial\xi}\cdot\Sigma(a\beta)g_{a\beta}v_\beta\frac{\partial x_a}{\partial\eta}}{\Sigma(ab)g_{ab}v_av_b}$$

(v_1, v_2, v_3 are the components of the velocity, and $v_4=1$).

In our problem the body is supposed to move in a normal system of co-ordinates. By this our formulæ simplify to [1]

$$\delta\int_{t_1}^{t_2}\int\left(-c^2\rho-U\right)\left(1-\frac{v^2}{c^2}\right)^{\frac{1}{2}}dtd\xi d\eta d\zeta=0, \quad . \quad (7)$$

$$\xi_\xi=\tfrac{1}{2}\Sigma(a)\left(\frac{\partial x_a}{\partial\xi}\right)^2+\frac{\left[\Sigma(a)v_a\frac{\partial x_a}{\partial\xi}\right]^2}{2(c^2-v^2)}-\tfrac{1}{2},$$

$$\xi_\eta=\Sigma(a)\frac{\partial x_a}{\partial\xi}\frac{\partial x_a}{\partial\eta}+\frac{\Sigma(a)v_a\frac{\partial x_a}{\partial\xi}\cdot\Sigma(a)v_a\frac{\partial x_a}{\partial\eta}}{c^2-v^2}$$

When applied to a revolving body, these equations will enable us to determine the deformation that is produced, wholly independently of the theory of relativity, by centrifugal force, a deformation that will in reality far surpass the changes we want to consider. To get free from it we can consider the ideal case of a "rigid" body—*i.e.* a body for which the moduli of elasticity A and B in (6) are infinitely great. The centrifugal force will then have no effect on the dimensions, but the changes required by the theory of relativity will subsist. The assumption has also the advantage of simplifying the calculations; indeed, since U becomes infinitely great, the term $-c^2\rho$ in (7) may be omitted.

I have worked out the case of a thin circular disc rotating with constant speed about an axis passing through its centre, at right angles to its plane. The result is that, if v is the velocity at the rim, the radius will be shortened in the ratio of 1 to $1-\frac{1}{8}\frac{v^2}{c^2}$. The circumference changing to the same extent, its decrease is seen to be exactly one-fourth of that of a rod moving with the same velocity in the direction of its length. That there would be a smaller contraction was to be expected; indeed, the case can be compared to what takes place when a hot metal band is fitted tightly around a wheel and then left to cool.

At first sight our problem seems to lead to a paradox. Let there be two equal discs A and B, mounted on the same axis, A revolving and B at rest. Then A will be smaller than B, and it must certainly appear so (the discs being supposed to be quite near each other) to any observer, whatever be the system of co-ordinates he chooses to use. However, we can introduce a system of co-ordinates S′ revolving with the disc A; with respect to these it will be B that rotates, and so one might think that now this latter disc would be the smaller of the two. The conclusion would be wrong because the system S′ would not be a normal one. If we leave S for it, we must at the same time change the potentials g_{ab}, and if this is done the fundamental equation will certainly again lead to the result that A is smaller than B.

[1] If in (7) we replace $(1-v^2/c^2)^{\frac{1}{2}}$ by $1-v^2/2c^2$, omitting the constant term $-c^2\rho$ and neglecting $U.v^2/2c^2$, we are led back to the ordinary formula (3).

The Geometrisation of Physics, and its Supposed Basis on the Michelson-Morley Experiment.

By SIR OLIVER LODGE, F.R.S.

SO much has been written about the Michelson-Morley experiment that it would be needless to refer to it here, had it not been interpreted by philosophic writers in an interesting but overviolent and, as some think, illegitimate manner. Historically it really does lie at the root of the remarkable attempt which is being made to geometrise physics, and to reduce sensible things like weight and inertia to a modification of space and time. The work of great Geometers has been pressed into the service, and a differential-invariant scheme of expression has been utilised to do for physics in general, and especially for gravitation, what Maxwell's equations did for

electric and magnetic forces. The prominent merit of these equations is that they replace any apparent predilection for forces acting at a distance, by explicit recognition of a modified medium (or at least a modified space) in contact with the accelerated particle.

The beauty and ingenuity of this scheme, and of the reasoning associated with it, are apt to overpower the judgment at times, and to lead to pseudo-philosophic conclusions which are not really justified, and some of which are repugnant to common-sense. It is scarcely wise to seek to interpret physically every link in a chain of mathematical reasoning. If the chain is coherent and if the terminal hooks are firm—that is, if the end results are intelligible and verifiable—no more need be expected from any system of equations. The astonishing thing is that statements of such extensive generality can be written in so compact a form, and that so considerable a range of experience can be summarised in terms of a pure hypergeometry, even though that be of a complex character.

It is undeniable that mathematicians, with a self-denying ordinance about coefficients, can thus attain remarkable criteria, and are able to anticipate definite results; but we need not seek to engraft their modes of expression on the real world of physics. We need not consider realities superseded, because a system of pure space and time can be devised which can formulate, and be consistent with, the movements observed by ordinary men and animals. What we observe is not motion and position alone: a wealth of colour, form, and beauty is also within our ken, and will not readily evaporate into a geometrical modification of empty space.

The Relativity argument is based on a policy of exclusion. It rejects everything that seems unnecessary; it dispenses with many of our long-standing conceptions; and accordingly is hailed as a simplification. The first simplification was the denial of any test for motion through a continuous fundamental medium, and a consequent ignoring of such a medium. The second step was to eliminate gravitational and other forces, with further denial of a power of discrimination between different kinds of acceleration. A third simplification, and further introduction of coefficients, enabled electro-magnetic forces to be similarly eluded. And if our conceptions permit of any further simplification, perhaps the additional properties of matter studied by Chemists and Biologists and Artists may be extruded too, and the rich fullness of the universe be impoverished into a mental abstraction.

To summarise, then:—

In such a system there is no need for Reality; only Phenomena can be observed or verified: absolute fact is inaccessible. We have no criterion for truth; all appearances are equally valid; physical explanations are neither forthcoming nor required; there need be no electrical or any other theory of the constitution of matter. Matter is, indeed, a mentally constructed illusion generated by local peculiarities of Space. It is unnecessary to contemplate a continuous medium as a universal connector, nor need we try to think of it as suffering modification transmitted from point to point from the neighbourhood of every particle of gravitational or electrified matter: a cold abstraction like a space-time-manifold will do all that is wanted, or at least all that the equations compel. And, as a minor detail, which will bring us to the point, it is not necessary to invoke a real Fitz-Gerald contraction in order to explain the result of the Michelson Experiment.

The Experiment.

With this prelude, which merely summarises a great deal more that in time I wish to say, let us proceed to the M.M. Experiment and recall attention to what it really does demonstrate rather than to what it is too often imagined to prove.

Take a horizontal square slab of stone (or wood), float it above an annular trough of mercury so that it can be steadily turned round a vertical axis. Fix mirrors to its surface, so arranged that a beam of light, split into halves, can be sent, one half to and fro lengthways, and the other half to and fro breadthways, each half travelling an equal distance as fixed by the slab. Reunite the two half-beams, observe the interference bands so formed, and see if they shift in a periodic manner during a leisurely rotation of the stone and observer through all azimuths.

If light is really a wave and not a projectile motion, and if we are living in a virtual drift of æther due to our real orbital motion through that medium, and if the stone or other material of the slab preserves and determines unalterably the actual distances travelled by the two half-beams of light in the interval between their split and their reunion—then the bands should undergo a sinuous shift, with amplitude representing a lag of the order 10^{-8}, as the stone revolves.

But, on the electrical theory of matter, cohesion is a residual chemical affinity or electrostatic attraction; and such attraction is known to be modified electrokinetically if the charged atoms are in rapid motion through the dielectric medium; hence it becomes not only possible but likely that the dimensions of the slab will change if we are moving through the æther; the square being subject to a slight distortion. And the change metrically to be expected, in accordance with electrical theory, is found on examination to be also of the order 10^{-8}, and in fact was ultimately shown by Larmor to be of a precisely compensating character. ("Æther and Matter," 1900, p. 175.)

The fact is that the lines of force on an isolated charged sphere redistribute themselves, if it be moving through the æther, as if it had become a stationary oblate spheroid with axis in the direction of motion. Such a spheroid becomes the static representation of a moving sphere, and may be held to take its place as the most symmetrical figure; for the only way in which we can infer the

virtual shape of an electron is by the distribution of its field.

Æther being incompressible, I expect that the change is really a pure constant-volume distortion, consisting of a pair of lateral extensions and a longitudinal contraction, as suggested in my B.A. address "Continuity," 1913, in the Birmingham volume, p. 25 (or in a separate publication by Dent and Son, pp. 58 and 111). But all that is significant for present purposes is the ratio of the longitudinal to the lateral change—a ratio commonly spoken of as the FitzGerald or Fitz-Gerald-Lorentz contraction.

It is customary to consider this as only a longitudinal shrinkage, but it is just as easy to allow for a possible lateral change too :—

The velocity of light through the medium being always c, and the speed of the light-conveying medium relatively to the matter of the block being v, the effective or resultant speed for the to-and-fro cross-current light-journey is $\sqrt{(c^2-v^2)}$, while for the up- and down-stream journeys the effective speeds are $(c-v)$ and $(c+v)$ respectively.

The intended perpendicular distances marked out on the slab being both x, the distance along-stream becomes, say, βx (whichever side of the slab gets periodically into that position as it rotates), and the distance across-stream may be called γx; so the respective single journeys are

$$\begin{array}{ll}(\text{along}) & \beta x=(c-v)t_1=(c+v)t_2\\ (\text{across}) & \gamma x=\sqrt{(c^2-v^2)}t_3.\end{array}$$

The observation does not consist in measuring either t_1 or t_2 or t_3; nor is even x measured with precision. The whole object of the experimenter is to measure the small excess,

$$t_1+t_2-2t_3,$$

to express it as a fraction of wave length, and to compare it with the distance $2x$. Repeated observations show that the excess is actually zero. And with this additional datum,

$$t_1+t_2=2t_3,$$

it is mere algebra to reckon that the necessary effective contraction is

$$\beta/\gamma=\sqrt{(1-v^2/c^2)};$$

while if we choose to add the constant volume relation, $\beta\gamma^2=1$, we get also

$$\gamma=(1-v^2/c^2)^{-\frac{1}{2}}.$$

The experiment therefore verifies the FitzGerald contraction, and tends to confirm the electrical theory of matter.

I was interested, when visiting the University of Chicago last winter, to find that Prof. Michelson himself was perfectly satisfied with this sort of view of his experiment, and did not consider that its interpretation necessitated any revolutionary considerations. The FitzGerald contraction is a peculiarity which could scarcely have been detected in any other way, since it is really an affair of the æther—the connecting medium in which all molecules are embedded—and affects every kind of matter to the same extent.

An objection has been raised, with apparent seriousness, that this contraction cannot be real, since, if it were, the rim of a spinning-wheel would contract more than the spokes, and so the ratio of circumference to diameter would not be π. This is an instructive and rather humorous example of the prevalent tendency to control physics by geometry. We might argue similarly that the rim of a wheel could not be a fraction of a degree cooler than the spokes; and it might be held—indeed it has been held—that the state of strain in an actual rotating wheel or disc would require non-Euclidean geometry to express it. If the propositions of geometry require physical measurements to sustain them, they can scarcely be of the kind we have been accustomed to associate with the name of Euclid. And if a special geometry has to be invented in order to account for a falling apple, even Newton might be appalled at the complications which would ensue when really difficult problems are tackled. Nevertheless, that is the kind of geometry to which relativity introduces us—a geometry based on hypothetical laboratory measurements with scales and clocks, and one the propositions of which can be interfered with by metrical observations. It is therefore sometimes called a "natural" geometry, free from metaphysics; it might equally well be called an abstract sort of theoretical physics, and not geometry at all.

Relativity Explanation of the Experiment.

It is well known that the simple interpretation above given of the M.M. experiment is not palatable to relativists; they consider that it is a forced and arbitrary explanation, and that they can account for the M.M. result more naturally by employing a geometrical device and by applying certain general hypotheses. The Principle of simple relativity is that a transformation to uniformly moving axes can make no difference to anything essential; and the accompanying obsession is that no observer can detect any apparent change in the velocity of light.

In order to apply these principles, the method adopted by a Relativist is to take two observers instead of one, to supply them with personal clocks and measuring rods, and then to make one of them fly through the laboratory at speed v; thus rendering accurate measurement rather difficult for him, and introducing some confusion into his ideas of space and time—especially as he is not to be allowed to know that he is moving. He may be at rest in the æther, but everything not attached to him or to his medium will be rushing along; accordingly objects will appear to be contracted, and all clocks but his own will seem to go slow.[1] The only thing

[1] It is not easy to explain without symbols why earthbound clocks should appear to go slow to an aviator, and an aviator's clock appear to go slow to a man on the ground. The plain man would think that they would both appear to go fast during approach, and slow during recession; but the meaning is not so simple as that. Nor is it because a pendulum has lengthened, or anything physical or real of that sort. The argument appears to be that the other man's clock must be estimated as relatively slow, by each of two observers moving relatively to each other, because otherwise they could measure different velocities of light; which, though not repugnant to common-sense, is contrary to the basic Principle of Relativity.

unaltered is speed, for he is allowed to compensate every confusion about space by an equal opposite confusion about time. However, he is provided with an instantaneous camera for taking snapshots which he can afterwards study, and he does his best; he makes his observations of times and distances, and records them with dashed letters. Meanwhile the other more comfortably situated observer, attached to the slab and the laboratory furniture (who also considers himself at rest, though the first man may think of him as rushing through the æther), records, undashed, the readings of his peculiar instruments too.

A relativist, not caring in the least which or whether either of these sets of measurements has any absolute meaning, but assuming them to be made accurately, applies the transformation of Lorentz and Larmor for change of co-ordinates between sets of axes moving relatively to each other; and thereby finds that if the laboratory readings are x and t, while the flying man's readings are x' and t' (each being supposed to have a local space and time of his own, absolute space and time having no meaning), the following relations must hold—

$$x' = \frac{x - vt}{\sqrt{(1 - v^2/c^2)}}; \quad t' = \frac{t - vx/c^2}{\sqrt{(1 - v^2/c^2)}}.$$

From these simple but important, and indeed fundamental, equations it comes out that if $x'/t' = c$ for the flying man, then $x/t = c$ for the other observer likewise. So the result is just as if nothing at all was moving, except light, and in fact as if bodily motion through the light-conveying medium were meaningless. Hence everybody should be satisfied, without any appeal to, or confirmation of, any physical theory whatever. The null (or dull) result of the experiment now requires no explanation; there is no need even to emphasise the double to-and-fro journey of the light; single journeys serve; for the apparent speed of light is (really, has been assumed) the same in all directions, no matter what it be referred to. Distances and times may appear different to different observers, but they are arbitrary or conventional appearances at best, and they tend to compensate each other.

Hence arises the $r = ct$ and $r' = ct'$ superstition, about the concentricity of a wave front round each one of a group of observers initially at the origin however much they may have scattered since; because, apart from gravitation, $x^2 + y^2 + z^2 - c^2t^2$ must be independent of axes of reference; or, what is the same thing in our simple case,

$$\frac{x - ct}{x' - ct'} = \frac{x' + ct'}{x + ct} = \text{constant} = \sqrt{\frac{c - v}{c + v}}.$$

And the other peculiarities of the simpler theory of relativity immediately follow: some of them very surprising and interesting.

For instance, take the composition of motions: If a thing is moving with speed $u = x'/t'$, relatively to an origin which itself is moving in (say) the same direction with speed v referred to a fixed origin, then the resultant speed $w = x/t$, referred to the same fixed origin, will be obtained by aid of the above transformation for moving axes reduced to relative rest—

$$x = \beta(x + vt); \; t = \beta(t' + vx'/c^2);$$

whence we get

$$w = \frac{x}{t} = \frac{u + v}{1 + uv/c^2}$$

a curious expression for the combined velocities. [Likening it to a rotation of axes, with $v/c = \tanh\theta$, the summation is not $\tanh\theta_1 + \tanh\theta_2$ but $\tanh(\theta_1 + \theta_2)$.] It gives simple summation for slow speeds, and an unattainable maximum c for high speeds. If either or both of the component velocities attain the magnitude c, the resultant w attains the same maximum, and cannot exceed it. But the most notable property of this expression is that it gives practically the same expression as Fresnel's æther-intuition gave, for light travelling down a stream of water—an expression verified by Fizeau's famous experiment. For let the speed u be that of light inside a dense material medium, say $c \div \mu$, and let the material medium be itself travelling in the same direction at speed v, then the resultant speed of the light would be, by the above expression,

$$w = \frac{c^2 + \mu vc}{\mu c + v},$$

or, what is the same thing,

$$w = c/\mu + v.\frac{1 - 1/\mu^2}{1 + v/\mu c}$$

And this, to a high degree of approximation, is practically identical with the Fresnel-Fizeau result, viz.:

$$w = c/\mu + v(1 - 1/\mu^2).$$

That such a result—which was supposed to give some kind of information about the behaviour of æther inside dense matter—can be obtained irrationally by a simple geometrical device, is surely surprising.

Again, the variation of the mass factor in momentum, which was originally predicted from the electrical theory of matter and afterwards verified, exhibits itself as an outcome of the relativity expression without any physical theory at all. The inertia of an electric charge, and indeed of all energy, seems to come out likewise. Light has a mass-factor, but not a conserved or invariable one, while matter has both a fixed and a variable term in its factor; which is a notable and suggestive pair of facts.

From the point of view of the above relativity composition of velocities, the Michelson-Morley result is obvious; for if the speed u under observation is already c, as it is in that experiment, it is useless to compound another velocity, $\pm v$, with it; because, if that is the law of composition, the resultant velocity still comes out c, no more and no less.

It may be said that whereas most experimenters assume an absolute time, the relativist assumes one absolute unattainable or unexceedable velo-

city. That is the assumption on which his whole argument is based; and, as an instinctive intuition, there may be a foundation for it (see below), and even for the further assumption that the fundamental absolute velocity is either equal to, or of the same order as, the velocity of light.

It must not be supposed however that the Michelson-Morley experiment substantiates the structure thus begun upon it. It does not really prove that the velocity of light in a moving medium is the same in all directions; that is a gratuitous but fundamental hypothesis, not really based on any experiment—certainly not on one which only deals with to-and-fro journeys over matter-fixed distances. Nevertheless, whether or not this foundation-stone of Relativity is well and truly laid, the structure built over it is of remarkable interest; and possibly can sustain itself by its own consistency, and by its attachments to other facts, even if it be admittedly arched over the particular experiment on which it was originally supposed to be founded.

A Few Remarks on the More General Theory of Relativity.

The consistency of all these things, including the conservation of energy and momentum, with simple relativity, amply accounts for the enthusiasm with which brilliant mathematicians, untrammelled by a sense of physical reality, have absorbed and developed the idea.

Indeed, as we know, Einstein was enticed to go on and try a transformation to accelerated axes also, to identify gravitation and inertia, to ally both with the so-called centrifugal force, and to reduce them all to a still more elaborate geometry, in which the Lorentz transformation is inadequate, and Euclidean propositions are superseded. He found ready to his hand a recondite scheme, provided for other and less worldly purposes by Gauss and Riemann; but, to make use of it, he had to introduce a fresh agnostic principle, a Principle of Equivalence. He must assume that we cannot discriminate between a whirling table and a gravitational field; that we have no criterion between a falling apple and a rising Earth; and that it is only prejudice which makes us feel assured about a rotating Earth, and unwilling to contemplate a diurnally revolving star. Geometrically they are all the same; and a suitable space-time system can be devised, and expressed in equations, which will account for, or at least express, most things observed in astronomy, and some additional things more allied with physics.

Now we must admit that if we are permitted to discard relativity as a *philosophy* and accept it as a *method*, the form of the most striking of its equations is advantageous, and represents an advance in symbolism, quite apart from any unphysical contributions to its origin and any lack of legitimacy about its birth. For the weight of an apple must be really, as it always has been hypothetically and vaguely, attributable to a gravitationally modified æther pressing down on each particle. Similarly the path of a planet from instant to instant can be expressed more intimately by a variational equation, involving always the next step, than by an action-at-a-distance formula, or by a merely kinematic summary which integrates the result over an entire orbit. But this powerful and ingenious method of research can be interpreted, in words, so as to suggest nothing more than a warping of an unreal emptiness—a sort of return to Descartes' vortices without their imagery, a rejection of Copernicus and Galileo and Newton.

Whereas what is really wanted for a truly Natural Philosophy is a supplement to Newtonian Mechanics, expressed in terms of the medium which he suspected and sought after but could not attain, and introducing the additional facts, chiefly electrical—especially the fact of variable inertia—discovered since his time. Such a philosophy would insist that the specific state of the medium throughout a gravitational field must be the immediate consequence, indeed the absolute essence, of the existence of each material particle; their separate potentials being combined by simple addition without any second-order complications. Equally an electric field must be part of the actual constitution of an electron, and inseparable from it: though the compounding of electric fields need not be so simple a matter, since charges in proximity do interfere with each other.

If we could understand the structure of the particle, in terms of the medium of which it is composed, and if we knew the structure of the rest of the medium also, so as to account for the potential stress at every point—that would be a splendid step, beyond anything accomplished yet. But that the particle is a singularity in the medium, and that its inertia and gravitational field are essential to, and part of, its very existence, must certainly be true; and this is what the Einstein theory, in its own peculiar geometrical unphysical way, has grasped. There are not half a dozen diverse but interlocked things in Nature: there is one definite quiescent medium, full of exceedingly fine-grained turbulent energy, with consequent properties which, when unravelled, will supply the key to all the non-mental phenomena occurring in it. Provided always that the locking up of small regions of this turbulence, not merely in the travelling form of light-waves, but in the potentially stationary form of electrons, can also be explained and understood.

Relativity, as a consecutive point-to-point method of arriving at results, is a first step towards this ideal, but it is not a Newtonian step; it is rather a blindfold method of investigation, like Entropy and Least Action.

The Fundamental Velocity.

To make a philosophic scheme of existence complete, more than the expression of a static instant is required, there must be duration likewise: the universe is not merely a Being but truly a Becoming; Time has to be associated with Space. This can only be done by a velocity factor of some kind; but to arrive at an appropriate factor, rela-

tivists have not dived down into the æther, as someone must ultimately dive, and dissected out the intrinsic speed of its turbulence—which is really the fundamental velocity in existence—they have utilised the more obvious and conspicuous *consequence* of this fundamental speed, viz. the uniform velocity with which the æther can convey a great variety of signals. Indeed their attachment to appliances like clocks and rods has led them to pay almost undue reverence to the æther's power of transmitting waves at a high and otherwise unattainable speed. It is true that these waves are among our methods of receiving and conveying information; but too much attention may be paid to the mere reception of information; and what is spoken of as "warping" is not limited to space alone. For some philosophers speak as if the duration of an event could be extended by merely delaying the reception of the news of its end; as if we could prolong a man's life by evading the tidings of his death, and might be entitled to say, without absurdity, that a man who died at seventy had lived seventy-one years and a lot of miles, if we had travelled so far that the messenger took a year to reach us. That such things can be gravely uttered is surely a tribute to the beauty and complexity of the mathematical scheme which can temporarily so warp the judgment even of the most competent.

If I am wrong in this I share a fraction of rashness with the admirable audacity of Einstein in the *Weltmacht oder Niedergang* sort of attitude which he takes up about his predicted shift of spectral lines. I feel a doubt whether those lines will be found shifted—at least when the observation is made outside a strong gravitational field—but I should be quite content either way, and would not think of asking anyone to abandon the method of relativity on that account.[2]

For undoubtedly general relativity, not as a philosophic theory but as a powerful and comprehensive method, is a remarkable achievement; and an ordinary physicist is full of admiration for the equations and the criteria, borrowed from hyper-Geometers, applied by the genius of Einstein, and expounded in this country with unexampled thoroughness and clearness by Eddington. But, notwithstanding any temptation to idolatry, a physicist is bound in the long run to return to his right mind; he must cease to be influenced unduly by superficial appearances, impracticable measurements, geometrical devices, and weirdly ingenious modes of expression; and must remember that his real aim and object is absolute truth, however difficult of attainment that may be, that his function is to discover rather than to create, and that beneath and above and around all Appearances there exists a universe of full-bodied, concrete, absolute, Reality.

[2] Since the above paragraph was in type a Circular, dated January, 1921, has reached me from Prof. Slipher, of the Lowell Observatory at Flagstaff, Arizona, recording an extravagant rate of recession—something like a thousand miles a second—for two specified nebulæ, presumably of the spiral class. But both nebulæ are reported to have large and brilliant nuclei; and if the concentration of their aggregate mass were sufficient—that is, if their M/R or $\rho^{1/2}$ were some two or three thousands of times greater than that of our sun, and therefore of a totally different order from that of our stellar system—the observation could be interpreted, not as recession, but as an Einstein shift of spectral lines.

Electricity and Gravitation

By PROF. H. WEYL.

[Translated by Dr. Robert W. Lawson.]

MODERN physics renders it probable that the only fundamental forces in Nature are those which have their origin in gravitation and in the electromagnetic field. After the effects proceeding from the electromagnetic field had been co-ordinated by Faraday and Maxwell into laws of striking simplicity and clearness, it became necessary to attempt to explain gravitation also on the basis of electromagnetism, or at least to fit it into its proper place in the scheme of electromagnetic laws, in order to arrive at a unification of ideas. This was actually done by H. A. Lorentz, G. Mie, and others, although the success of their work was not wholly convincing. At the present time, however, in virtue of Einstein's general theory of relativity, we understand in principle the nature of gravitation, and the problem is reversed. It is necessary to regard electromagnetic phenomena, as well as gravitation, as an outcome of the geometry of the universe. I believe that this is possible when we liberate the world-geometry (on which Einstein based his theory) from an inherent inconsistency, which is still associated with it as a consequence of our previous Euclidean conceptions.

The great accomplishment of the theory of relativity was that it brought the obvious principle of the *relativity of motion* into harmony with the existence of *inertial forces*. The Galilean law of inertia shows that there is a kind of obligatory guidance in the universe, which constrains a body left to itself to move with a perfectly definite motion, once it has been set in motion in a particular direction in the world. The body does this in virtue of a tendency of persistence, which carries on this direction at each instant "parallel to itself." At every position P in the universe, this tendency of persistence (the "guiding field") thus determines the infinitesimal parallel displacement of vectors from P to world-points indefinitely near to P. Such a continuum, in which this idea of infinitesimal parallel displacement is determinate, I have designated as an "affinely connected" one (*affin zusammenhängend*). According to the ideas of Galileo and Newton, the "affine connection" of the universe (the difference between straight and curved) is given by its geometrical structure. A vector at any position in the universe determines directly and without ambiguity, at every other position, and by

itself (*i.e.* independently of the material content of the universe), a vector "equal" to itself. According to Einstein, however, the guiding field (*Führungsfeld*) is a physical reality which is dependent on the state of matter, and manifests itself only infinitesimally (as a tendency of persistence which carries over the vectors from one point to "indefinitely neighbouring" ones). The immense success of Einstein's theory is based on the fact that the effects of gravitation also belong to the guiding field, as we should expect *a priori* from our experience of the equality of gravitational and inertial mass. The planets follow exactly the orbit destined to them by the guiding field; there is no special "gravitational force" necessary, as in Newton's theory, to cause them to deviate from their Galilean orbit. In general, the parallel displacement is "non-integrable"; *i.e.* if we transfer a vector at P along two different paths to a point P′ at a finite distance from P, then the vectors, which were coincident at P, arrive at P′ in two different end-positions after travelling these two paths.

The "affine connection" is not an original characteristic of the universe, but arises from a more deeply lying condition of things—the "metrical field." There exists an infinitesimal "light-cone" (*Lichtkegel*) at every position P in the world, which separates past and future in the immediate vicinity of the point P. In other words, this light-cone separates those world-points which can receive action from P from those from which an "action" can arrive at P. This "cone of light" renders it possible to compare two line-elements at P with each other by measurement; all vectors of equal measure represent one and the same *distance* at P. In addition to the determination of measure at a point P (the "relation of action" of P with its surroundings), we have now the "metrical relation," which determines the congruent transference of an arbitrary distance at P to all points indefinitely near to P.

Just as the point of view of Einstein leads back to that of Galileo and Newton when we assume the transference of vectors by parallel displacement to be integrable, so we fall back on Einstein when the transference of distances by congruent transference is integrable. But this particular assumption does not appear to me to be in the least justified (apart from the progress of the historical development). It appears to me rather as a gross inconsistency. For the "distances" the old point of view of a determination of magnitudes in terms of each other is maintained, this being independent of matter and taking place directly at a distance. This is just as much in conflict with the principle of the *relativity of magnitude* as the point of view of Newton and Galileo is with the principle of the *relativity of motion*. If, in the case in point, we proceed in earnest with the idea of the continuity of action, then "magnitudes of condition" occur in the mathematical description of the world-metrics in just sufficient number and in such a combination as is necessary for the description of the electromagnetic and of the gravitational field. We saw above that, besides inertia (the retention of the vector-direction), gravitation was also included in the guiding field, as a slight variation of this, as a whole, constant inertia. So in the present case, in addition to the force which conserves space- and time-lengths, *electromagnetism* is also included in the metrical relation. Unfortunately, this cannot be made clear so readily as in the case of gravitation. For the phenomena of gravitation are easily obtained from the *Galilean principle*, according to which the world-direction of a mass-point in motion follows at every instant the parallel displacement. Now it is by no means the case that the ponderomotive force of the electromagnetic field should be included in our Galilean law of motion, as well as gravitation, for a charged mass-point does not follow the guiding field. On the contrary, the correct equations of motion are obtained only by the establishment of a definite and concrete law of Nature, which is possible within the framework of the theory, and not from the general principles of the theory.

The form of the law of Nature on which the condition of the metrical field is dependent is limited by our conception of the nature of gravitation and electricity in still greater measure than it is by Einstein's general principle of relativity. When the metrical connection alone is virtually varied, the most simple of the assumptions possible leads exactly to the *theory of Maxwell*. Thus, whereas Einstein's theory of gravitation gave certain inappreciable deviations from the Newtonian theory, such as could be tested by experiment, our interpretation of electricity—one is almost tempted to say unfortunately—results in the complete confirmation of Maxwell's laws. If we supplement Maxwell's "magnitude of action" (*Wirkungsgrösse*) by the simplest additional term which also allows of the virtual variation of the "relation of action," we then arrive at *Einstein's laws of the gravitational field*, from which, however, there are two small deviations:

(1) That *cosmological term* appears which Einstein appended later to his equations, and which results in the spatial closure (*Geschlossenheit*) of the universe. A hypothesis conceived *ad hoc* by Einstein to explain the generally prevailing equilibrium of masses results here of necessity. Whereas Einstein has to assume a pre-stabilised harmony between the "cosmological constant" which is characteristic for his modified law of gravitation, and the total mass fortuitously present in the universe, in our case, where no such constant occurs, the world-mass determines the curvature of the universe in virtue of the laws of equilibrium. Only in this way, it appears to me, is Einstein's cosmology at all possible from a physical point of view.

(2) In the case where an electromagnetic field is present, Einstein's cosmological term must be supplemented by an additional term of similar

character. This renders the existence of charged material particles possible without requiring an immense mass-horizon as in Einstein's cosmology.

At first the *non-integrability of the transference of distances* (*Streckenübertragung*) aroused much antipathy. Does not this mean that two measuring-rods which coincide at one position in the universe no longer need to coincide in the event of a subsequent encounter? Or that two clocks which set out from one world-position with the same period will possess different periods should they happen to encounter at a subsequent position in space? Such a behaviour of "atomic clocks" obviously stands in opposition to the fact that atoms emit spectral lines of a definite frequency, independently of their past history. Neither does a measuring-rod at rest in a static field experience a congruent transference from moment to moment.

What is the cause of this discrepancy between the idea of congruent transfer and the behaviour of measuring-rods and clocks? I differentiate between the determination of a magnitude in Nature by "persistence" (*Beharrung*) and by "adjustment" (*Einstellung*). I shall make the difference clear by the following illustration: We can give to the axis of a rotating top any arbitrary direction in space. This arbitrary original direction then determines for all time the direction of the axis of the top when left to itself, by means of a *tendency of persistence* which operates from moment to moment; the axis experiences at every instant a parallel displacement. The exact opposite is the case for a magnetic needle in a magnetic field. Its direction is determined at each instant independently of the condition of the system at other instants by the fact that, in virtue of its constitution, the system *adjusts* itself in an unequivocally determined manner to the field in which it is situated. *A priori* we have no ground for assuming as integrable a transfer which results purely from the tendency of persistence. Even if that is the case, as, for instance, for the rotation of the top in Euclidean space, we should find that two tops which start out from the same point with the same axial positions and encounter again after the lapse of a very long time would show arbitrary deviations of their axial positions for they can never be completely isolated from every influence. Thus, although, for example, Maxwell's equations demand the conservational equation $de/dt=0$ for the charge e of an electron, we are unable to understand from this fact why an electron, even after an indefinitely long time, always possesses an unaltered charge, and why the same charge is associated with all electrons. This circumstance shows that the charge is not determined by persistence, but by adjustment, and that there can exist only *one* state of equilibrium of the negative electricity, to which the corpuscle adjusts itself afresh at every instant. For the same reason we can conclude the same thing for the spectral lines of atoms. The one thing common to atoms emitting the same frequency is their constitution, and not the agreement of their frequencies on the occasion of an encounter in the distant past. Similarly, the length of a measuring-rod is obviously determined by adjustment, for I could not give *this* measuring-rod in *this* field-position any other length arbitrarily (say double or treble length) in place of the length which it now possesses, in the manner in which I can at will predetermine its direction. The theoretical possibility of a determination of length by adjustment is given as a consequence of the *world-curvature*, which arises from the metrical field according to a complicated mathematical law. As a result of its constitution, the measuring-rod assumes a length which possesses this or that value, *in relation to the radius of curvature of the field*. In point of fact, and taking the laws of Nature indicated above as a basis, it can be made plausible that measuring-rods and clocks adjust themselves exactly *in this way*, although this assumption—which, in the neighbourhood of large masses, involves the displacement of spectral lines towards the red upheld by Einstein—does not appear anything like so conclusive in our theory as it does in that of Einstein.

The Relativity of Time.

By PROF. A. S. EDDINGTON, F.R.S.

THE philosopher discusses the significance of time; the astronomer measures time. The astronomer goes confidently about his business and does not think of asking the philosopher what exactly is this thing he is supposed to be measuring; nor does the philosopher always stop to consider whether time in his speculations is identical with the time which the world humbly accepts from the astronomer. In these circumstances it is not surprising that some confusion should have arisen.

In many globular clusters there are stars which oscillate in intrinsic brightness; let us select two such stars from different clusters and invite all the astronomers in the universe to measure the true interval of time between the moments of maximum light of the two stars. They must, of course, make whatever measurements and calculations they consider necessary to allow for the finite velocity of light. It may easily happen that the astronomers on Arcturus report that the two maxima were simultaneous; whereas those on the earth report an interval of *ten years* between the same two maxima. There is here no question of observational error; the recognised terrestrial method necessarily gives a discordant result when used on Arcturus, owing to its different motion.

Our first impulse is to blame the astronomers.

Evidently they are not giving us the true time-interval; and now that they are informed of the discordance they ought to give up their out-of-date procedure. But the astronomers reply: "Tell us, then, how we ought to find this 'true time.' By what characteristics are we to recognise it?" No answer has been given. Michelson and others sought in vain for an answer; for if our velocity through the æther could be defined, it would single out one universal system of time-measurement which might reasonably (if somewhat arbitrarily) be called true. Meanwhile the phrase *true time* is a "meaningless noise." It is idle to contest with those who hold that the thing exists and ought to be regarded. "Who would give a bird the lie, though he cry 'Cuckoo' never so?"

The direction of Northampton measured by astronomers at Cambridge is due west; measured by astronomers at Greenwich it is north-west. It is no use to tell them that they must adopt a different plan, and find a "true direction" of Northampton which does not show these discordances. They reply: "We are perfectly aware that there must be discordances, as you call them; but that is in the nature of a relative property like direction; as for this true direction which shall be the same from all stations, we have no idea what you are talking about."

The time determined by astronomers and in general use is thus a fictitious time, or, in the usual phrase, it is *relative* to terrestrial observers. Similarly it has been found that extension in space is also relative. When the Copernican theory led to the abandonment of the geocentric view of the universe, the revolution did not go far enough; it was thought that we could pass to the heliocentric outlook by merely allowing for what in pure geometry would be called a change of origin. Actually a more profound transformation is necessary. For example, the Michelson-Morley experiment is a terrestrial experiment, but its theory is treated from a heliocentric point of view; that is to say, account is taken of the varying orbital motion of the earth; it furnishes a proof of the famous FitzGerald contraction, and much ingenuity has been spent on an electrical explanation of this curious property of matter. Einstein's theory waves this aside with the remark: "Of course, your results appear strange when you describe the apparatus in terms of a space and time which do not belong to it. Your electromagnetic discussion is no doubt valid, but it is leading you away from the root of the matter; the immediate explanation lies in the difference between the heliocentric and geocentric space and time systems."

It was shown by Minkowski that all these fictitious spaces and times can be united in a single continuum of four dimensions. The question is often raised whether this four-dimensional space-time is real, or merely a mathematical construction; perhaps it is sufficient to reply that it can at any rate not be less real than the fictitious space and time which it supplants. Terrestrial observers divide the four-dimensional world into a series of sections or thin sheets (representing space) piled in an order which signifies time; in other words, the enduring universe is analysed into a succession of instantaneous states. But this division is purely geometrical. The physical structure of the enduring world is not laminated in this way; and there is nothing to prevent another observer drawing his geometrical sections in a different direction. In fact, he will do so if his motion differs from ours.

Now it may seem that we have been paying too much deference to the astronomers: "After all, they did not discover time. Time is something of which we are immediately conscious." I venture to differ and to suggest that (subject to certain reservations) time as now understood *was* discovered by an astronomer—Römer. By our sense of vision it appears to us that we are present at events far distant from us, so that they seem to occur in instants of which we are immediately conscious. Römer's discovery of the finite velocity of light has forced us to abandon that view; we still like to think of *world-wide* instants, but the location of distant events among them is a matter of hypothetical calculation, not of perception. Since Römer, time has become a mathematical construction devised to give the least disturbance to the old illusion that the instants in our consciousness are world-wide.

Without using any external senses, we are conscious of the flight of time. This, however, is not a succession of world-wide states, but a succession of events at one place—not a pile of sheets, but a chain of points. Common-sense demands that this time-succession should be essentially different from the space-succession of points along a line. The preservation of a fundamental distinction between timelike succession and spacelike succession is essential in any acceptable theory. Thus in the four-dimensional world we recognise that there are two types of ordered succession of events which have no common measure; type A is like the succession of instants in our minds, and type B is the relation of order along a line in space. Proceeding from the instant "here-now," I can divide the regions of the world into two zones, according as they are reached by a succession of type A (my absolute past and future), or of type B (my absolute "elsewhere"). This scheme of structure is very different from the supposed laminated structure of the older view. Since we believe that this distinction of types A and B corresponds to something in the actual structure of the world, it is likely to determine the various natural phenomena that are observed. Thus it determines the propagation of light, since it is found that the line of a light-pulse is always on the boundary between the two zones above-mentioned. More important still, a particle of matter is a structure which can occupy a chain of points only of type A. Since we are limited by our material bodies, it must be this type of succession which we immediately experience; we are aware

of the existence of the other type only by deduction from the indications of our external senses.

Objection is sometimes raised to the extravagantly important part taken by light-signals and light-propagation in Einstein's discussion of space and time. But Einstein did not invent a space and time depending on light-signals; he pointed out that the space and time already in general use depended on light-signals and equivalent processes, and proceeded to show the consequences of this. Turning from fictitious space and time to the absolute four-dimensional world, we still find the velocity of light playing a very prominent part. It is scarcely necessary to offer any excuse for this. Whether the substratum of phenomena is called *aether* or *world* or *space-time*, one requirement of its structure is that it should propagate light with this velocity.

The resolution of the four-dimensional continuum into a succession of instantaneous spaces is not dictated by anything in the structure of the continuum. Nevertheless, it is convenient, and corresponds approximately to our practical outlook on the world; and it is rarely necessary to go back to the undivided world. We have to go back to the undivided world when a comparison is made between the phenomena experienced by observers with different motions, who make the resolution in different directions. Moreover, a world-wide resolution into a space and time with the familiar properties is possible only when the continuum satisfies certain conditions. Are these conditions rigorously satisfied? They are not; that is Einstein's second great discovery. It is no more possible to divide the universe in this way than to divide the whole sky into squares. We have tried to make the division, and it has failed; and to cover up the consequences of the failure we have introduced an almost supernatural agency—gravitation. When we cease to strive after this impossibility—a mode of division which there was never any adequate reason for believing to be possible—gravitation as a separate agency becomes unnecessary. Our concern here is with the bearing of this result on time. Time is now not merely relative, but local. The relative time for an observer is a construction extended by astronomers throughout the universe according to mathematical rules; but these rules break down in a region disturbed by the proximity of heavy matter, and cannot be fulfilled accurately. We can preserve our time-partitions only by making up fresh rules as we require them. The local time for a particular observer is always definite, and is the physical representation of the flight of instants of which he is immediately aware; the extended meshwork of co-ordinates radiating from this is drawn so as to conform roughly to certain rules—so as not to violate too grossly certain requirements which the untutored mind thought necessary at one time. Subject to this, time is merely one of four co-ordinates, and its exact definition is arbitrary.

To sum up, world-wide time is a mathematical system of location of events according to rules which on examination can only be regarded as arbitrary; it has not any structural—and still less any metaphysical—significance. Local time, which for animate beings corresponds to the immediate time-sense, is a type of linear succession of events distinct from a pure spacelike succession; and this distinction is fully recognised in the relativity theory of the world.

Theory and Experiment in Relativity.[1]

By Dr. Norman Campbell.

"SPACE" and "time" are the conceptions of theory, not of laws. They are neither necessary nor useful in the statement of the results of any experiment. The experimental concepts with which, like all theoretical ideas, they are connected are such magnitudes as length, area, volume, angle, period (of a system), or time-interval. The numerical laws of experimental geometry involve two or more "spatial" magnitudes and no other magnitudes; for example, the area of a rectangle is proportional to the product of the lengths of its sides. There are no laws relating "temporal" magnitudes only.

Relativity neither adds to nor subtracts from the collection of spatial and temporal laws. The laws which it explains all involve magnitudes that are not spatial or temporal. And this is fortunate. For the subject has been so completely examined that it is very improbable that any proposed new laws could be true. If relativity predicted anything inconsistent with firmly established experiment, Nature would not devote a special number to discussing it.

It may be objected that relativity does predict new and strange laws; it predicts that the velocity of light in a region remote from material bodies is always the same; and it predicts unfamiliar experiences of observers travelling at great speeds or in the neighbourhood of concentrated mass. But, it may be replied, the measurement of the velocity of light does not involve only spatial and temporal magnitudes; we do not measure that velocity as we do the velocity of a material body; an element of theory is always involved. Again, we do *not* observe any disturbance of geometrical laws in the neighbourhood of the densest bodies we know. And as for Prof. Eddington's observers in aeroplanes travelling with half the velocity of light, no two human

[1] Since it is impossible to make a short article on a large subject anything but a summary, perhaps I may be permitted to refer any reader who is interested to my "Physics: The Elements" for a fuller discussion of many of the questions raised.

beings have smoked, or, if the doctrines of relativity are true, ever will smoke, cigars—let alone make accurate measurements—in such aeroplanes, and afterwards compared their experiences. If we pretend to talk about experiments, let us be sure that we do talk about experiments, and not about something that cannot possibly happen.

However, it may not be useless to ask what would happen if we did find our spatial laws untrue, in the manner suggested, at speeds that can be realised. I suggest that we should make our laws true once more by changing slightly the meaning of the terms in them. The technical terms of science are labels attached to collections of observations that can be grouped into laws, which those terms are used to describe. If we find that the supposed laws are not true, the terms become meaningless; we might abandon them altogether; but generally we discover that, by a slight regrouping of the facts according to the new laws, we can make once more a collection of the facts to which the old term may be applied appropriately to state the new laws in almost precisely the old form.

Consider, for example, the term "simultaneous." Primarily, two events were judged to be simultaneous by direct perception. Using this test and examining a limited range of experience, we found the law that events that are simultaneous to one observer are simultaneous to another. But later we found that the law was not valid for more extended experience, including the sound and flash from a gun. That discovery made "simultaneous" meaningless, and with it all the temporal magnitudes; there was no longer any way of assigning uniquely a numeral to represent the time-interval between two events. So we changed the meaning of "simultaneous," and introduced a "correction" (very complicated, as sound-rangers know); by this means we made "simultaneous" once more the expression of a law, and reproduced our specifications for measuring time-intervals in exactly the old form, but, of course, with rather different content. If we encountered new difficulties when we extended our observations to events in systems moving with great relative speeds, we could, I think, introduce a new "correction" for speed, and reproduce once more the form of our old laws and our old methods of measurement. At any rate, the resulting change of form need not be so great as to cause any appearance of paradox.

I conclude, therefore, that nothing that the most extravagant imagination has suggested so far could make us diverge appreciably from our present spatial and temporal laws. But it is otherwise with our theories. The experimental physicist has a theory of time and space, although he may not be conscious of it. It is based on Cartesian geometry.[2] It likens "space" to an array of black dots in a cubical lattice, and "time" to a series of ticks from a metronome. It connects the position of a body with the individual characteristics of the dots that it "occupies," and the magnitudes length, area, volume with the number of those dots. The time of an event it connects with the individual characteristics of the ticks. The theory explains well some spatial laws, but in some directions it is misleading. Thus it fails to make a distinction between lengths and areas, which (in the last resort) must be measured by the superposition of rigid bodies,[3] and volumes, which cannot be measured by such superposition. It should be noted that the dots and ticks, the "points" and "instants" of mathematically minded philosophers, are purely theoretical ideas. They have no meaning apart from the theory, and, like the position of a hydrogen molecule, cannot be determined by experiment.

Prof. Einstein has altered and expanded this theory. In conjunction with Minkowski, he has altered it by merging the dots and ticks, formerly independent, into a single array of world-points, and by making the arrangement of these points quite different from that of a cubic (or Euclidean) lattice. He has expanded it by introducing the idea of the "natural path" of a body among the points, which enables him to explain the laws of dynamics without the (theoretical) idea of forces. But his propositions still form a theory, and they still contain purely theoretical ideas, which cannot be determined by experiment—the world-point or the infinitesimal "interval," which must be integrated before it can be related to measured magnitudes.

These changes are very disturbing to the experimenter. He wants theories to explain laws. Explanation involves not only the possibility of deducing the laws (for that is easily attained), but also the introduction of satisfactory ideas. In the older types of physical theory this "satisfactoriness" was obtained by means of an analogy between the ideas of the theory and the concepts of some experimental laws. Thus in the older theory of space the points were related in a way analogous to that in which small material bodies can be related. In the new theory this analogy fails. For the mathematician the passage from flat three-dimensional space to curved four-dimensional space is trivial; for the experimenter it is vital, because we do not actually experience any arrangements at all analogous to those of points in such a space. The satisfactoriness of the theory, for those who press it on our attention, is derived, not from material analogy, but from the intrinsic elegance and beauty of the relations involved, the faculty for appreciating which distinguishes the pure mathematician from his fellows.

It is not surprising, therefore, that experimenters have found difficulty in accepting the theory as an ultimate solution of their problem. The old theories explained, because they inter-

[2] It is interesting to notice that, though the theory is sometimes called Euclidean, Euclid had never heard of it. No Greek geometer would have known what you meant if you had told him that space was three-dimensional.

[3] "Rigid bodies" is a label attached to a collection of facts grouped in laws, the laws that make possible the measurement of lengths and areas.

preted in terms of familiar ideas; even to the most revolutionary of mankind, familiarity is a source of some satisfaction. The new theory is based on ideas utterly unfamiliar, and it might be urged that anything based on them must be the precise contrary to explanation. But if we ask why we are so ready to accept theories based on material analogy, we shall find our reason in the fact that such theories have actually turned out to possess the amazing property of predicting unsuspected laws. The theory of relativity also possesses that property. Ought we not to extend, so as to include it, our notions of the proper limits of physical theory, and to rid ourselves of the discomfort of unfamiliarity by the simple process of studying its ideas so closely that they become an integral part of our mental equipment?

It may be asked, Do theories, indeed, aim at nothing but satisfactoriness and prediction? Is not their object rather to discover the true nature of the real world? Such questions must be answered by questions. Do physicists (I say nothing of mathematicians or philosophers) believe that anything is real for any reason except that it is a conception of a true law or of a true theory? Have we any reason to assert that molecules are real except that the molecular theory is true—true in the sense of predicting rightly and interpreting its predictions in terms of acceptable ideas? What reason have we ever had for saying that thunder and lightning really happen at the same time, except that the conception of simultaneity which is such that this statement is true makes it possible to measure time-intervals? When these questions are answered it will be time to discuss whether relativity tells us anything about real time and real space.

The Relation between Geometry and Einstein's Theory of Gravitation.

By DOROTHY WRINCH and DR. HAROLD JEFFREYS.

THE term "geometry" has been used ever since the time of Euclid to denote two completely distinct subjects; but the formal similarity of their propositions has been so close as to obscure until recently the entire dissimilarity of their status in scientific knowledge. The Greek geometers seem to have been inspired originally by the need for a satisfactory method of surveying; at the same time, their logical turn of mind led them to present their results in the now familiar form of a deductive science. The characteristics of such a science are that a certain number of primitive propositions p_1, now called postulates, are stated at the beginning, and that from these, by a process of pure logic, further propositions q_1 are one by one developed. But this development is quite a separate process from that of deciding whether the primitive propositions are true or not, and if this is not done it is impossible to assert that the deduced propositions are true.

Different sets of primitive propositions p_2, p_3, . . . would give different sets of deduced propositions q_2, q_3, . . . and the complete working out of these is a science in itself; its results are all, therefore, of the form "p_1 implies q_1," "p_2 implies q_2," and so on. Euclid actually used in his development several postulates which he never explicitly stated, but which have been made explicit by modern writers; our present object, however, is not to indicate these, but to consider his geometry in the perfectly deductive form it would have had if he had actually stated them. We have noticed that in any other system in which any one of Euclid's postulates is false, many of his deduced propositions are also false. This, however, does not affect his method in the least; all his arguments are independent of the truth of the postulates, and in every case it is possible to assert—and this is all the modern geometer asserts—that *if* the postulates are true the propositions are true. A system like Euclid's is, therefore, a part of pure logic; the large division of pure logic that includes it as a very special case is pure geometry. Of the many systems of pure geometry now known, all are on just the same footing, and there is no sense in which any one of them is preferable to any other.

Euclid's contemporaries, however, were not interested merely in his logical method; they wished to identify the furrows in their fields with his lines, and the fields themselves with his surfaces; and to have some justification for this it was necessary to assume that his postulates were true of them. Only one example is needed to show how formidable an assumption this was. In order to prove one of his earliest propositions, Euclid assumes that a triangle can be picked up, transported bodily, and deposited on top of another. Imagine this process carried out when the triangles are fields! The impossibility of carrying it out implies that a most important proposition was not proved for the very case to which they contemplated applying his geometry, and hence that, so far as the knowledge of that day went, there was not the slightest reason for believing that geometry was applicable for its original purpose of earth-measurement. Yet its results, in so far as they were capable of being applied in actual surveying, seem to have been instantly accepted. Why? It may have been due partly to lack of disposition to criticise something that the critics felt they could not have done better themselves, a mental attitude that may perhaps still occasionally exist; but the chief reason was probably that some of the deduced propositions were directly verifiable, such as the proposition that the equality of corresponding

sides of triangles in pairs implies that of corresponding angles; also many propositions about areas, expressed in numerical form, could be verified with some accuracy. As a result of these successes the whole system was accepted. But this could not have been a demonstration of the truth of the system; however many times a congruence proposition is verified for particular pairs of triangles, it will never be possible to prove it true for the next pair without the further assumption of some *principle of empirical generalisation* that is not included among the postulates.

Thus the grounds on which Euclid's results have been generally adopted in practice are not those of logical deduction from postulates known to be true *a priori*, as was for centuries believed; they are largely based on empirical generalisation. Thus two sciences with different fundamental data, but with many formally similar propositions, have grown up: the original name of *Euclidean geometry* will here be retained for Euclid's own development, while the experimental science of measurement will be called *mensuration*. The latter was not developed on its own account, the reason being probably that it is a physical science, and that so long as a theory gives results in accordance with observation, physicists in general show little disposition to investigate the security of its foundations.

All experimental science depends on some postulate or postulates that imply that empirical generalisation is justifiable: no amount of experiment will enable us to make any inference unless we have some principle that enables us to generalise the results. Theories involving such a principle may be called *extensive*, while those not involving one may be called *intensive*. The latter include the whole of logic. Now no process of generalisation is used in pure geometry; every proposition is proved immediately and with complete certainty for every instance of its terms. Thus all pure geometry, Euclidean or otherwise, is intensive, while all physical sciences, including mensuration, are extensive. Thus, in the first great subdivision of scientific knowledge, geometry and mensuration fall on opposite sides, and nothing but confusion can arise from any attempt to treat them as identical. In geometry postulates are made about *any* point, or *any* line; in mensuration these are necessarily unverifiable, for they would have to be tested for every possible instance before they could be treated as postulates. They can thus be obtained only by generalisation from results that *are* obtained by experiment, and therefore cannot possibly be primitive propositions.

It may be remarked in passing that generalisation was condemned by traditional logic. The fact that scientific men have not studied its validity for themselves is the chief reason for the present chaotic condition of the theory of scientific knowledge. This important and basic principle appears to involve necessarily the notions of *probability* and *combination of observations*; yet in works purporting to be theories of scientific knowledge these topics are habitually ignored altogether or else relegated to the last chapter. Much credit is due to Dr. N. R. Campbell for placing the theory of probability in its proper position in his recent "Elements of Physics." The value of his treatment is injured by the adoption of the Venn definition of probability, which is logically unsound and scientifically inapplicable; but to accord probability its true status in scientific knowledge is the first great advance in the theory.

This neglect of mensuration, while geometry was making rapid progress, has led physicists to give undue attention to the latter subject and an undue physical status to its concepts, especially to that of *space*. Metrical geometry has come to be regarded as the theory of the measurement of space; but space has no status among the subject-matter of mensuration. A physical measurement of length is always of the form: "When the zero mark on the (so-called rigid and straight) scale is in contact with the particle A, and the edge of the scale is in contact with the particle B, B lies between the nth and $(n+1)$th divisions of the scale." This involves no reference to space. Even the notion of rigidity, which is often regarded as spatial, is not so in practice. It is an experimental fact that many bodies exist which under ordinary treatment have the property that, if the distance between two points of the one can be made to include the distance between two points of the other, in some configuration, then, however the bodies are displaced, this remains true; and our rigid scales are such bodies. This, with the purely mechanical process of scale dividing, is all that is needed to make measurement possible. In actual physical calculation, again, we simply use the measures themselves without any reference to "space"; and the final result can be stated wholly in terms of such measures. If the notion of space is introduced at any stage of the investigation, it necessarily eliminates itself before the close. There are, of course, many so-called measurements of length in physics, such as the diameter of the earth and the distance of the sun, which cannot be made by means of rigid scales; these are not, in the strict sense, measured at all, but inferred from measures of certain angles by means of physical laws obtained from other experiments. Time, as actually used in physics, is on a similar footing to distance measures, and not to space.

The relations between the measured positions of bodies at different times form the subject of dynamics, which reduces to mensuration in a special case. It is an extensive science, and therefore epistemologically quite different from any four-dimensional geometry, which must from its nature be intensive. For this reason we consider that the theory of gravitation must be treated on extensive lines, and disagree with Prof. Eddington's presentation of Einstein's theory as a section of geometry. The principle of the irrelevance of the mesh-system, in particular, does not seem to be presented in its true light in his

"Space, Time, and Gravitation." This principle states that there are relations between the coefficients in the formula for ds^2 which hold, no matter what system of co-ordinates is chosen. Eddington regards the co-ordinate system as decided upon arbitrarily, having no real physical importance, and always eliminating itself in any actual physical process, while the only thing that has physical importance is space-time, which he treats on purely geometrical lines. From this point of view the principle is evidently correct; but it is not the point of view of physics. In physics the co-ordinate systems actually chosen are adopted entirely because they give specially simple forms to relations between measured quantities, and thus are not chosen arbitrarily. It would therefore be conceivable, and indeed not improbable, that there could be no relations between the g's that would not have special forms with ordinary co-ordinate systems. On the other hand, the properties of space-time never appear in physical laws; thus it is space-time that eliminates itself when the problems are reduced to terms of measurement, and the irrelevance of the mesh-system is a proposition, not about the unimportance of convention, but about physical measurements themselves. Hence it is a part, not of geometry, but of dynamics, and can be arrived at only by extension. Now that the predictions of the theory have been verified, the proper course to adopt is to derive the form of ds^2 from the experimental results that these express, and to regard the principle as an interesting experimental result.

Einstein's own presentation differs somewhat from Eddington's, but is also open to some objections. In his "Relativity, the Special and the General Theory" (English translation) Einstein states on p. 2 that he uses the term "geometry" in its usual sense of the logical connection of ideas (? propositions) among themselves. Then, on p. 3, he introduces the further proposition that two points on a practically rigid body always correspond to the same distance, however the body may be displaced, and considers that this converts geometry into a branch of physics. This is not the case; it can only give series of implications between propositions of the truth of which we are ignorant, until some of these propositions have been proved by sensory experience, which is not a part of geometry; they also require a process of generalisation to yield results of sufficient generality to form the basis of a geometry, so that a science reached in this way must be extensive.

On p. 9 of his book, Einstein's attitude towards "space" is closer to ours than to Eddington's, for he resolves to shun the word entirely, admitting that he cannot form the slightest conception of its meaning, and replaces it by the notion of position (*i.e.* measured position) relative to a practically rigid body of reference. In the latter part of the book he appears to regard space, not as a primary entity of Nature, but merely as a conventional construct, composed of the aggregate of all possible values of the three position co-ordinates. In this form the notion may be useful in theoretical work, but we cannot attribute any ultimate physical importance to a thing we have constructed ourselves. In fact, he makes it clear on p. 60 that the principle of relativity is a postulate to be tested by experience: a suggestion offered as possibly true, but with no *a priori* necessity about it. Its validity, in the opinion of its author, rests wholly on the success of its physical predictions. Consequently, as was stated above, the correct procedure now is to deduce the theory from the facts originally predicted by it, with whatever further postulates may be necessary.

Although we have criticised the chief current expositions of the Einstein theory, it is the omission of physicists to provide any satisfactory analysis of the foundations of their own subject that is chiefly at fault, and the remedy is a proper discussion of the relation of physical laws to the observations on which they are founded, and of the probability of inferences based upon them.

We may devote some attention to the question of the comparison of standards at different places, which must play an important part in any theory of measurement. Before the publication of the special theory of relativity, the accepted view was comparatively simple: the measured length of the conventional rigid bar was supposed to be the same however it was displaced and turned, and the same applied to the period of vibration of a clock or an atom (in the former case subject to the known influence of temperature and gravity). This gave a convenient basis for comparison of standards, for all could be expressed in terms of some standard instruments. The special theory, however, showed that this statement is not merely inconvenient as a working rule, but also demonstrably false, for a bar must have different measured lengths according as it is moving relative to the observer along or across the direction of its length. The possibility of a comparison was restored by Einstein's system of light signals; time and length standards were supposed unaltered by displacement, provided the observer was moving with them. But whether he was moving with them or not, a certain interval $\int ds$, measured between consecutive vibrations of the clock or between points on the bar, remained unaltered. Also the condition that $\int ds$ was stationary for small variations in the path was found to give the conditions satisfied by a particle moving at a great distance from matter.

Thus this differential element ds had a dual importance in the special relativity theory. In the general theory this is generalised, but it may be noticed that it is just possible that the two *rôles* may really be separated in a gravitational field. The consequences of this would be peculiar. It is well known that the assumption that ds, taken through a period of vibration of an atom, is independent of position, leads to the prediction of a shift of solar spectral lines; though this is so mixed up with shifts arising from other causes

that it is impossible to say definitely whether it has been observed or not. But it is not so well known that the assumption that would lead to no spectral shift leads to the result that the wave-length on emission of light from a particular type of atom is a function of position; thus the abandonment of *ds* as the fundamental measured quantity would not make it possible to carry both length and time standards about unaltered. Probably the difficulties arising from the hypothesis that *ds* does not play two parts are so great as to render it quite unplausible.

The Metaphysical Aspects of Relativity.

By PROF. H. WILDON CARR.

THERE is a possible misconception in the application of the term "metaphysical" to the new principle of relativity which it is advisable to clear up. In the great era of the triumphant advance of the positive sciences, which began about the end of the first third of the nineteenth century, metaphysics was decried as the main obstacle to scientific progress. Following the lead of Auguste Comte, the workers in the sciences held it up to scorn as obscurantism. The derision and reproach which were then poured on it have clung to it ever since. There are many to-day who acknowledge, indeed, that metaphysics must be assigned a place in the hierarchy of the sciences, but interpret the Aristotelian definition, "that which follows or comes after physics," as indicating a dark realm of the yet unknown, or even of the unknowable, which surrounds the clear zone of positive knowledge, into which we may peer, but will discern nothing. The objects of metaphysics—the soul, the cosmos, the deity—are in this view vain imaginings, not objects of which there can be knowledge in the scientific meaning—that is, objects amenable to the experimental method. Such a view simply ignores the scientific tradition. Modern science is the result of the formulation and adoption of the experimental method, but the experimental method is not self-evident or inherently rational; it depends on a metaphysical concept, and its rationality can be established only by metaphysical principles. To contrast, then, the experimental method with the principles on which it depends, to describe one as the realm of science and the other as the realm of ignorance or unknowability, is from any philosophic point of view stultifying, and, in the literal sense, absurd.

What has made it possible to consider metaphysics as an unreal science, or as a realm of unreal fancy, is the peculiar position in regard to the natural sciences in which the purely mathematical sciences stand. Mathematics does not use the experimental method, and in the hierarchy of the sciences mathematics seems sufficient of itself for the foundation and support of the whole superstructure. But mathematics is only an abstract science of quantity; its concepts lack the one essential character which experimental science calls for—concreteness—and this metaphysics alone can supply.

The modern era of philosophy from Descartes onwards has been dominated by the insistence of the scientific problem—that is, the problem of the ultimate nature of the reality we study in physical science by the experimental method. This interest in the nature of scientific reality replaces the main interest of the philosophy of the mediæval period, which was concerned with the origin and destiny of the human soul, and, more generally, with the relation of man to God. If modern philosophy may be said to join hands with the ancient philosophy of Greece, it is not in the identity of its interest; for, though the Greeks were mathematicians, they had no conception of the experimental method as we practise it, and it is even doubtful if it could have been made to appeal to them on the ground of rationality.

The principle of relativity is the direct outcome of the application of the experimental method, and the full force of its appeal is based on our absolute confidence in the metaphysical concept of reality which is the ground and reason of that method. The experimental method has taken possession of the modern mind, and it assumes for us something like the unmodifiable character of an instinct. If experiment proves a certain velocity to be constant under conditions which require us to predict its variation; if experiment shows the movement of a source of light to be without the expected effect on the velocity of propagation—well, it is our concept of the nature of reality which must adapt itself to the experiment. The prediction is based on the concept that space and time provide an absolute system of reference; the null result of the experiment negatives that concept, and henceforth space and time are "shadows"; they must vary, because under varying conditions velocity is constant.

Those who affirm that the principle of relativity is purely mathematical, and not metaphysical, and, therefore, resent the intrusion of metaphysics into the discussion of its equations, conceive the principle to be purely methodological, to be concerned only with abstract quantitative measurement, and merely to substitute a very complex and difficult set of equation-formulæ for a discarded simpler one, in the interest of greater precision and accuracy alone. Those who take this view seem to me to misapprehend the significance of the principle. It is to be understood only when taken in its historical connection with the metaphysical constructions of the great philosophers.

Since Descartes, the speculations of philosophy have centred round the concepts of substance and

cause, and the principle of relativity in its two phases, special relativity (the restricted theory) and general relativity, is essentially concerned with these two concepts. The first phase, in its negativity towards the æther hypothesis, is a reform of the notion of substance; and the second, in its rejection of influence and its substitution of equivalence for attraction in a new theory of gravitation, is a reform of the notion of cause.

Two opposing principles in regard to both these concepts—substance and cause—have been struggling to establish themselves throughout the modern period—one taking as its type the objective or passive aspect presented by the world to the mind of the observer, the other taking as its type the subjective activity of the mind itself in perceiving, imagining, understanding, willing, and acting. The first type we have in Descartes' concept of material substance as consisting in extension alone, and in his concept of cause as the mechanical action and interaction of a definite quantity of movement imparted to the extended substance—the concept of a mechanism which embraces the whole universe, organised and unorganised, exclusive only of the other substance, thought or thinking, present in human beings alone. Later we have the same type in the more familiar concepts of Newton—absolute time and absolute space. "Absolute time, in itself, and from its own nature, flows equally, without relation to anything external." "Absolute space, in its own nature, without relation to anything external, remains always similar and unmovable." The other type of concept we have in Leibniz's monadology. Substance is not passive, but active; cause is not movement, but force. What does nothing is nothing. Time and space are *ordines rerum non res*. Things are centres of active force.

It is with these concepts of substance and cause that the principle of relativity is primarily and mainly concerned, and these concepts are metaphysical constructions. Experimental facts have called for the formulation of the principle, but those facts themselves have slight importance in the practical sphere; it is their theoretical consequences which are far-reaching and revolutionary. They are facts which prove to be decisive in regard to metaphysical problems. The experiments are concerned with such infinitesimals as forty-two seconds in relation to a century, or a variability of 2¾ in. in the diameter of the earth. It is not the facts themselves, therefore, that are important, but their significance. According to the view which I have put forward in my book, "The General Principle of Relativity in its Philosophical and Historical Aspect" (Macmillan and Co.), the principle of relativity definitely decides for us that our universe is monadic, and that our science does not derive its validity from a reality independent of the monads, but from a power inherent in the monads to co-ordinate ever-varying points of view. By monads I mean minds, but minds conceived as metaphysical reals.

The point of supreme and central importance in the principle of relativity in its bearing on metaphysics is its negative attitude to the concept of absolute space and absolute time continua. The principle accepts the null result of the experiments as decisive in regard to the non-reality in the physical sense of such continua, and it refuses to recognise any necessity to construct *ad hoc* a hypothetical absolute space-time system. On the other hand, it claims to provide a formula which expresses the identity of an event for two observers in different systems who pronounce it to be one and the same, without the necessity of affirming an absolute order independent of their systems of reference.

Why does this seem paradoxical and in contradiction to our ordinary experience? Because our experience consists in the observation of events which we do not cause; which we refer to in our intercourse with our fellows as common to them and to us; and to which throughout life we, automatically or consciously, react. We argue by what appears to us the most perfectly natural reasoning that the identity of an event for two different observers implies an absolute order by reference to which alone differences of observation can be reconciled. This absolute order, we think, can be nothing else but the determination of every event in regard to every other event in an absolute coexistence in space and in an absolute succession in time. We conceive, therefore, an absolute space-time order, and suppose our private space-time systems are related to it. Such is the course of reasoning which appears natural, and such is the logical necessity from which it appears impossible to escape. Metaphysicians have long disputed it, but their arguments have been generally set aside as logomachies. Experiment has now falsified it.

What sort of thing, then, is the relativist universe? Substance and cause—that is, the principle of unity and the principle of uniformity—are definitely transferred from the object to the subject of experience. I do not mean that object and subject are dissociated; I mean that substance and cause are declared to be functions of the essential activity, and not of the passivity of experience. Thus the universe depends on the subject of experience, not, indeed, in the old and often derided sense in which the philosopher is caricatured as evolving an external world out of his own inner consciousness, as the spider spins its web, but in the sense that the universe is the co-ordination which the observer effects. The universe has four dimensions—the three dimensions of space, and the one dimension of time. The principle of co-ordination is that every observer uses his own axes of dimension, taking his system of reference as fixed in relation to all systems which for him are moving, and he is able to do so because his four axes are variable, and every change in his own system of reference, relatively to other systems, is compensated by a variation in his axes of co-ordination which preserves the ratio constant.

The universe, then, which the principle of rela-

tivity affirms is a universe in which there is no absolute space-time order; in which every event is exhausted in the contradictory descriptions of observers in different systems of reference; in which systems of reference are ultimate without being absolute, and relative without being externally conditioned; in which every system is self-sufficing and contains its own norm, a norm which remains constant by changing as the system changes. In such a universe, are mathematics and physical science possible? The relativist claims that they are capable of infinitely greater precision and consistency than they could ever attain while obstructed by the old concept.

Bibliography of Relativity.

A BIBLIOGRAPHY of all books, pamphlets, papers, articles, and other publications on the subject of relativity has been prepared by Dr. H. Forster Morley, director of the International Catalogue of Scientific Literature. The list includes nearly 650 titles, arranged in chronological order from 1886 to the end of last year. It would occupy about thirty columns of NATURE, and, much as we should like to print it in full, limitations of space render this impossible. We have, therefore, extracted from Dr. Morley's bibliography the titles of published books and pamphlets upon relativity and related subjects, and also the references to articles, notes, or other contributions which have appeared in the pages of NATURE. The complete bibliography is so valuable that we trust it will be published in full either by a scientific society or in a leading work on relativity. Dr. R. W. Lawson has kindly added the titles of a number of German works.

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Notes.

THE new session of Parliament was opened by the King in person on Tuesday. Reference was made in the King's Speech to a measure to be introduced into the House of Commons "to deal with the safeguarding of essential key industries of the country, and with certain aspects of unfair and abnormal industrial competition."

A MEETING of the organising committees of all the Sections of the British Association will be held at Burlington House on Friday, February 25, to consider possible arrangements for joint programmes and related matters for the Edinburgh meeting of the Association in September next. Dr. Ethel Thomas, recorder of Section K (Botany), asks us to say that she would be glad to hear as soon as possible before February 25 from those who propose to offer papers for the Edinburgh meeting, and no doubt the recorders of other Sections would like to receive similar early notice.

SUPPORT for the appeal for the protection of the optical glass industry which appeared as a leading article in NATURE of February 10 comes in the form of a series of resolutions passed by the Optical Society at its general meeting on the same date. The importance of optical munitions is emphasised, and it is pointed out that such supplies can be maintained only if the necessary materials, manufacturing establishments, and skilled labour are available. No other industry can by adaptation of existing resources produce the class of work required. The supply of optical glass required by this country should also be ensured. Another point made is that although the world's pre-war demand for optical glass was only about 25 tons per annum, some sixty different varieties are required; experience has shown that manufacture under these conditions cannot be a commercial proposition. In conclusion, it is urged that, in the interest of research, no obstacle should be allowed to prevent men of science from obtaining the instruments necessary for their work. The society is convinced that the optical industries of this country must be preserved and developed, and the President of the Board of Trade is to be asked to receive a deputation to explain more fully the position of these industries.

THE gold medal of the Royal Astronomical Society was presented on February 11 to Prof. H. N. Russell for his work on the theory of star-development. The retiring president, Prof. A. Fowler, recalled in his address that Prof. Russell had introduced them in that very room to the giant and dwarf theory nearly eight years ago. Although a theory on the same lines (the division of the stars into two groups in which the temperature is rising and falling respectively) had been proposed many years earlier by Sir Norman Lockyer, it ran so counter to the then accepted views that he (Prof. Fowler) acknowledged that he had remained unconverted for some time; but recently several lines of evidence have converged to support the truth of the theory, namely, (1) the accumulation of additional parallaxes, both visual and spectroscopic; (2) the theoretical researches of Prof. Eddington and others on the physics of giant stars and the correlation of temperature with mass; and (3) the recent optical confirmation of the immense bulk of the typical red giant star Betelgeux. There is now little dispute as to the main lines of the theory, though there is room for much further work, in which Prof. Russell is taking a prominent part, on the problem of the nebulæ and Wolf-Rayet stars and their place in the scheme of development. Prof. Russell, in returning thanks, expressed his indebtedness to Prof. E. C. Pickering for determining the spectral type of his parallax stars, and noted that Prof. Hertzsprung and himself had put forward the theory simultaneously.

IN recent years M. l'Abbé Breuil has contributed to *L'Anthropologie* a series of memoirs on the rock-paintings of Spain. In the last of these (June, 1920) he sums up the general results to which his studies have led him, and states his belief that all known examples can be arranged in an evolutionary series dating from the closing phases of the Ice age to the dawn of the Neolithic period. In a lecture given at the Institut Français, Cromwell Gardens, S.W., on February 9, the Abbé exhibited a series of original slides illustrating the remarkable art which flourished in southern France and throughout the Spanish Peninsula during the age of the Reindeer, and summed up for his hearers the conclusions reached after many years of systematic investigation. He believed that in the region covered by the art of the cave-men three centres or localities of independent or semi-independent development could be distinguished, North Spain and South-West France forming one centre, East Spain a second, and South Spain a third. The paintings and engravings in the first and second of these cave-areas were, he believed, the work of different peoples, although both were hunters, both giving graphic and realistic representations of the animals they followed and captured, the mammoth, reindeer, bison, and horse. In South Spain, on the other hand, the influence of a pastoral people was to be observed on cave art; it is in this centre that the final degradation of the earlier realistic art of the north into the later linear, conventional form of the south could be detected. There seems reason to believe that it may be possible to trace the evolution of an alphabetical system from the original graphic art of the earlier cave-men. The cave art of South Spain has many points in common with the art of the Bushmen of South Africa.

OWING to the recent decision of the Government that, for the present, no new schemes which involve the expenditure of public money shall be proceeded with, the Ministry of Agriculture and Fisheries informs bee-keepers that no further progress can be made at present with the Bee Disease Bill which was introduced into the House of Lords on December 20 last. The Ministry has also been making preliminary arrangements for the establishment of a Bee Advisory Committee which, it was hoped, would represent

thoroughly every section of the bee-keeping industry; its function would be to advise the Ministry on all apicultural matters, including the question of legislation respecting bee diseases. In view of the need for economy and for the postponement of legislation, it will be necessary to delay the formation of this Committee. In the meantime, it is hoped that bee-keepers will take steps to organise themselves in order to arrive at a means of expressing an agreed opinion on the measures to be taken for the protection and furtherance of the industry. Arrangements for the examination of diseased bees no longer exist at the Department of Comparative Anatomy, The Museum, Oxford. Pending the appointment by the Ministry of a bacteriologist to undertake such work, Dr. J. Rennie has kindly consented to carry out examinations and to furnish reports to the bee-keepers concerned. In future, therefore, specimens, together with the name and address of the sender, should be sent to Dr. J. Rennie, Marischal College, University of Aberdeen.

SIR ROBERT HORNE, President of the Board of Trade, in opening on February 10 the Efficiency Exhibition organised by the *Daily Mail* at Olympia, referred to the importance of science and research to industry. In the past we had been too much accustomed to develop business without regard for the sciences or arts; now that attitude is passing away, and the sciences, arts, and industry are working well together. As examples of the success which has attended the co-ordination of effort, Sir Robert mentioned the Dorman appliances for the transmission of wave-energy through water, and the progress which has been made in preserving the health of the nation. We are compelled to defer an account of some of the exhibits of scientific interest until next week, but wish now to direct particular attention to the stand organised by London University, on which Prof. Fleming has arranged a number of the original valves and lamps used by him when he was evolving the thermionic valve. The series enables a view to be obtained of the genesis of one of the most valuable of modern inventions, which was itself the outcome of purely scientific research.

SIR ERNEST SHACKLETON, the *Times* announces, is planning a two years' expedition to Arctic regions, and proposes to leave England in May or June next, accompanied by a dozen men chosen chiefly from those who accompanied him on former expeditions. He proposes to sail for Hudson's Bay, where 150 dogs will be embarked, and then to proceed, *via* Baffin's Bay, through Lancaster Sound to Axel Heiberg Land. The passage of Lancaster Sound and Barrow Strait and the course to be followed to the northward must depend largely on ice conditions. The main object of the expedition is said to be to explore the region between Axel Heiberg and the Parry Islands. Parts of this area were explored between 1898 and 1902 by Otto Sverdrup's expedition in the *Fram*, when surveys were made by G. Isachsen of the coasts of Axel Heiberg, Ellef Ringnes, and Amund Ringnes Lands. Stefansson, travelling from Prince Patrick Island, visited the region in 1915 and 1916, and discovered two new islands in the Gustav Adolf Sea and one in the Prince Gustav Sea, besides finding that Ellef Ringnes Land is in reality two islands. The American Crocker Land Expedition under Mr. D. B. Macmillan in 1916 reached King Christian Island, travelling south-westward from Ellesmere Land. A great deal of work, however, remains to be done, and there is little doubt that Sir Ernest Shackleton intends to push eastward into the unknown Beaufort Sea, where land possibly exists, although Peary's Crocker Land has been disproved and Stefansson has found a relatively narrow continental shelf. It is reported that Sir Ernest Shackleton has bought the Norwegian whaling vessel *Foca I.*, which will shortly leave Tromsö for England. For some years this vessel has been used in carrying stores and men to Spitsbergen for a Norwegian mining company.

SIR LYNDEN MACASSEY in his address on February 11 to the Institute of Industrial Administration upon present-day industrial psychology attributed our present industrial discontent (i) to the failure of Capital to give sufficient attention to the question of administration, and (ii) to the lack of economic information, both general and particular to their respective industries, among the workers. He pleaded for a revaluation of the factors of production so as to reach a nobler conception of the responsibilities and possibilities of administration. It will be generally agreed that in the past far too much has been left to chance, and that the time has come when this side of our industrial life must be completely overhauled. There may, perhaps, be less general agreement with Sir Lynden's view that a second condition of securing efficiency in production is that Labour shall be regarded as entitled to "know the facts," obtained from an efficient costing system, about the industries in which it is engaged. Yet facts of this kind have infinitely greater weight than an unsupported statement of the management in which, unfortunately, the men have probably little or no faith. Sir Lynden is inclined to underestimate the extent to which welfare work is regarded with suspicion, and his suggestion for its extension will meet with considerable criticism from those who are demanding that they shall be allowed an increased share in all branches of management.

THOUGH the various British firms which are specialising in the manufacture of organic research chemicals continue to add from time to time to the range of products made, their lists are still far behind those issued by the German firms before the war in the variety and complexity of organic chemicals offered. The slow progress made is due to difficulties with which all chemists have become familiar since 1914, and not the least of these is the uncertainty of the Government policy towards the fine chemical industry as a whole. Meanwhile, research work must go on, and since the date of the Sankey judgment research chemists have again begun to make use of German sources of supply, which, it may be added, are no longer so abundant, cheap, and various as they were in 1914, though they appear to be rapidly improving. As it seemed possible that the Dyestuffs (Import

Regulation) Act, 1920, might interfere with the importation of such chemicals, the council of the Institute of Chemistry addressed a letter to the Board of Trade inquiring whether licences would be necessary for the import of small quantities of organic chemicals (including intermediate products used in the manufacture of dyes, etc.) required solely for research purposes. The Board of Trade in its reply states that whilst it is not possible to regard small quantities of organic intermediate products which may be required for research purposes as being outside the scope of the Act, the Board will be prepared to issue general licences for the importation of such products to approved research institutions, covering periods of three months and limited only as to total quantities. This procedure obviates the necessity of separate applications for a large number of small items, but it will be a condition of the issue of such a general licence that a detailed return shall be furnished, at the end of the three months during which the licence is in operation, of the quantities of each product actually imported under it. This ruling indicates that the authorities are prepared to interpret the Act in such a way as not to hamper the prosecution of research.

THE Parkin prize, value 100*l.*, will be offered by the Royal College of Physicians of Edinburgh for the best essay "On the Effects of Volcanic Action in the Production of Epidemic Diseases in the Animal and in the Vegetable Creation, and in the Production of Hurricanes and Abnormal Atmospheric Vicissitudes." Essays intended for competition, which is open to competitors of all nationalities, must be written in English, and must reach the secretary not later than December 31, 1921. Each essay must bear a motto, and be accompanied by a sealed envelope bearing the same motto outside and the author's name inside.

THE Department of Scientific and Industrial Research has approved the formation of a Research Association for the Cast Iron and Allied Industries as complying with the conditions laid down in the Government scheme for the encouragement of industrial research. The secretary of the committee engaged in the establishment of this association is Mr. Thomas Vickers, 174 Corporation Street, Birmingham.

WE learn from the *British Medical Journal* of February 12 that an institute of biology named after Ramon y Cajal has been established at Madrid. It consists of four sections, devoted to human and comparative histology, neurology, physiology, and experimental pathology respectively.

Our Astronomical Column.

IONISATION IN THE SUN.—Two papers by Dr. Megh Nad Saha have recently appeared in the *Phil. Mag.* (vol. xl., pp. 472, 809), in which the author suggests an explanation of the varying character of the spectra exhibited by different elements in the sun. It is well known that some elements are represented by both enhanced and arc lines. Others show enhanced lines only, while a few have not been found at all. Prof. Fowler has ascribed these variations very largely to the different atomic weights of the elements concerned, but Dr. Saha is of the opinion that atomic weight is only of minor importance, being largely balanced by selective radiation pressure, and regards the variations mentioned above as arising from the different responses of elements to the stimulus existing in the sun. The enhanced lines are assumed to be due to radiations from "ionised atoms" (*i.e.* atoms that have lost an electron) rather than to a mere increase in temperature, and the different ionisation potentials of the elements will thus cause their varying behaviour. Calculations are given of the percentage ionisation of various elements at different temperatures and pressures. The results certainly agree quite well with the observed differences of behaviour of the elements chosen, and also with their individual variations in different regions of the sun.

THE FIRST VOYAGE ROUND THE WORLD.—Prof. Pio Emanuelli, of the Vatican Observatory, contributes an article to the *Corriere d'Italia* of December 20 last on the voyage of Magellan four hundred years ago. Magellan sailed from Spain on September 20, 1519, with five ships. He stayed in Brazil for some months, during which, as his biographer Pigafetta tells us, the potato was tasted for the first time by Europeans.

On the resumption of the voyage to the south the Straits of Magellan were discovered and traversed, the ships reaching the Pacific on November 28, 1520. On April 27, 1521, Magellan and several of his companions were treacherously slain in the Island of Zebu, in the Philippines. Some of the ships were captured, while others had to be abandoned owing to insufficient crews. Only one ship, the *Victoria*, returned to Spain, arriving on September 6, 1522, with the 18 survivors of the expedition, which had originally numbered 237.

Prof. Emanuelli notes that the first clear and definite description of the Magellanic clouds dates from this expedition. Thus they deservedly perpetuate Magellan's name in the heavens. Attention is also directed to the fact that until this voyage the geography of the earth as a whole was less well known than that of the planet Mars at the present day.

SECULAR CHANGE IN THE PERIOD OF δ CEPHEI.—Prof. Eddington, in studying the pulsation theory of the Cepheids, indicated that a fairly rapid secular change of period was to be expected, and referred to Hertzsprung's conclusion that the period of δ Cephei was diminishing by 0·079s. per annum. Mr. H. Ludendorff gives a further discussion of the early observations in *Ast. Nach.*, No. 5076. He has carefully re-reduced the original observations of Mr. F. M. Schwerd, made between 1823 and 1826, and finds that they accord closely with the change of period announced by Hertzsprung. The formula of the latter gives a shift of the time of maximum amounting to $-0{\cdot}0121$ of a period in 1825, while Schwerd's observations give $-0{\cdot}0134 \pm 0{\cdot}005$. The following values of the shift as given from other early series of observations are deduced:—Goodricke, 1785: shift $-0{\cdot}036 \times$ period $\pm 0{\cdot}005$; Pigott, 1785: shift $-0{\cdot}035 \times$ period $\pm 0{\cdot}011$; Westphal, 1818: shift $-0{\cdot}010 \times$ period $\pm 0{\cdot}008$. Mr. Ludendorff considers that the reality of the change is placed beyond doubt by this discussion.

University and Educational Intelligence.

CAMBRIDGE.—As was generally expected, Report B in favour of the foundation of a separate women's university at Cambridge was rejected last week. Three of the six signatories, who produced the scheme after six months' labour, had signed its death-warrant by issuing a fly-sheet against it. The new scheme—C it may well be called—is supported now by a group of fifty-two. It offers women students degrees, teaching by the University for a limited number, also certain undefined privileges, but no power and no votes in the Senate or any controlling body of the University. Even this scheme seems to open up too many and too serious risks to the more conservative members of the University, who are appealing for support for a fourth scheme D. While rigidly limiting the number of women students admitted to the University and ensuring that they shall pay proper fees for teaching and examinations, this scheme limits any advance from the present position to the granting by diploma of titular degrees. Already several defections have taken place from the moderate party to the extreme conservative wing.

What might have been an agreed solution in 1897 can scarcely be accepted in that light in 1921, and unless the moderates, having shed their right wing, are prepared to go further than their fly-sheet suggests, it seems probable that the intervention of some outside body—the Royal Commission at present sitting on the financial resources of the University—will be sought.

THE annual prize distribution at the Sir John Cass Technical Institute was held on Thursday, February 10, when the prizes were distributed by Sir Frederick Black, who afterwards delivered an address on "Liquid Fuel in Peace and War." A summary of the work of the institute during the past session shows that the total number of students was 1060—a higher figure than in any previous year, and an increase of more than 50 per cent. on the previous session. An important development contemplated was the initiation of courses of instruction on petroleum technology adapted to the needs of those already engaged in the industry. In the course of his address Sir Frederick Black said that whilst the United Kingdom had become an important consuming centre of petroleum products, it was at present only on a comparatively small scale that petroleum was produced in this country. So far as manufacture or refining was concerned, much more work of that nature was likely to be undertaken at home, for large British companies interested in oil had their headquarters and distributing agencies here. After briefly describing how the products of petroleum used for fuel were obtained, Sir Frederick gave a general description of their use in internal-combustion engines and for steam raising. The war has developed enormously their use and established their importance. The relative advantages of oil and coal for marine purposes were discussed, special reference being made to the progress in the building of motor-driven ships since 1912. Great attention was being given to such important matters as the elimination of waste on the oilfields and in the use of liquid fuels. Oils that admitted of complete refining into such products as motor spirit and lighting and lubricating oils should be so dealt with in preference to burning the more valuable fractions for steam raising, provided that a heavier oil not capable of such complete refining could be made available.

Calendar of Scientific Pioneers.

February 17, 1600. Giordano Bruno died.—An enthusiastic supporter of the views of Copernicus and of other new learning, Bruno, after several years spent in visiting France, England, and Germany, returned to his native country, was arrested, and, refusing to recant his philosophical and scientific heresies, was burnt at the stake in the Campo di Fiori, Rome.

February 17, 1867. Alexander Dallas Bache died.—One of the most influential of American men of science, Bache was director of the United States Coast Survey. He played a leading part in the founding of the National Academy of Sciences.

February 17, 1874. Lambert Adolphe Jacques Quetelet died.—The designer and first director of the Brussels Observatory, Quetelet was also a statistician and applied the theory of probabilities to the physical and intellectual qualities of man.

February 17, 1875. Friedrich Wilhelm August Argelander died.—Trained under Bessel, Argelander in 1837 became professor of astronomy at Bonn, where he compiled his great catalogue and atlas containing 324,000 stars visible in the northern hemisphere.

February 18, 1899. Marius Sophie Lie died. February 19, 1897. Karl Theodor Wilhelm Weierstrass died.—Both Lie and Weierstrass were among the most prominent workers in pure mathematics of last century. The former, though a Norwegian, for some years held a chair of mathematics at Leipzig, while Weierstrass was long connected with Berlin University. Lie's chief work was on the theory of transformation groups; Weierstrass's on elliptic functions and the theory of functions.

February 20, 1762. Johann Tobias Mayer died.—Director of Göttingen Observatory, Mayer left behind him valuable lunar tables which greatly aided in solving the problem of finding the longitude at sea.

February 20, 1907. Henri Moissan died.—Moissan in 1886 was the first to isolate fluorine. He improved the electric furnace, and by suddenly cooling a solution of carbon in molten iron produced small artificial diamonds.

February 22, 1875. Sir Charles Lyell died.—Lyell was the greatest master of English geology, and the publication of his "Principles of Geology" marked an epoch in the history of that science. This work was "an attempt to explain the former changes of the earth's surface by reference to causes now in operation." Lyell is buried in Westminster Abbey.

February 22, 1901. George Francis FitzGerald died.—Erasmus Smith professor of natural philosophy at Trinity College, Dublin, FitzGerald is remembered for his knowledge and versatility, his brilliant conceptions, and his stimulating influence on his fellow-physicists.

February 23, 1812. Etienne Louis Malus died.—A French military engineer of distinction, Malus died at the age of thirty-six, having three years previously made his great discovery of the polarisation of light by reflection.

February 23, 1855. Karl Friedrich Gauss died.—Mathematics, astronomy, optics, and magnetism all engaged the attention of Gauss, who in 1807 became professor of mathematics and director of the observatory at Göttingen. His important investigation of terrestrial magnetism belongs to the latter part of his life. With Weber he invented new instruments, set up an electric telegraph, and built the first magnetic observatory. The Gauss Tower on the Hohenhagen, near Dransfeld, was erected to his memory in 1911.

E. C. S.

Societies and Academies.

PARIS.

Academy of Sciences, January 24.—M. Georges Lemoine in the chair.—The president announced the death of M. Georges Humbert, member of the Academy.—P. **Termier** and L. **Joleaud**: The age of the phenomena of transport in the mountains of Gigondas (Vaucluse).—G. **Gouy**: A theorem of geometrical optics and its application to systems of prisms.—M. Pierre Bazy was elected a member of the section of medicine and surgery in succession to M. Guyon.—J. **Andrade**: The transverse elastic displacements of the centre of gravity of a cylindrical spiral and its doublets.—C. **Féry**: A mechanical clock with free escapement.—A. **Liénard**: The electromagnetic energy and thermodynamic potential of a system of currents.—G. **Reboul**: A new property of bodies poor conductors of electricity. An attempt to discover the causes of phenomena described in an earlier communication.—M. **Collignon**: The propagation of the sound of guns to great distances. The annual periodicity. Confirmation of results given in a previous communication: the law of annual periodicity has been verified up to November 18, 1918.—L. **Forsén**: The systematic nomenclature and constitution of the derivatives of molybdic acid. Representing molybdic acid by the formula

$$H_6Mo_5O_{12}.(3H_2MoO_4),$$

the composition of the complex salts of molybdates can be satisfactorily represented.—E. **Rengade**: The isothermal concentration of a solution prepared starting with two salts with different ions. A study of the compositions of the solution and deposited salts produced by the isothermal evaporation at 25° C. of a solution of ammonium nitrate and sodium chloride.—Mlle. J. **Bonnefoy** and J. **Martinet**: 6-Methylisatin. Two new methods are given for the preparation of this compound, the first starting with metatoluidine and methyl mesoxalate, and the second with metatoluidine, carbon bisulphide, and hydrogen peroxide.—E. E. **Blaise**: Derivatives of 1 : 4-diketones and semicarbazide. The primary product of the reaction between the diketone and semicarbazide is a disemicarbazone; this is readily converted in formic acid solution into an *N*-ureo-pyrrol.—G. **Mignonac**: A new general method for the preparation of amines, starting with aldehydes or ketones. The aldehyde or ketone is dissolved in an alcoholic solution of ammonia, finely divided nickel added, and the mixture kept violently shaken, treated with hydrogen at atmospheric pressure. Examples of the application of the method are given.—P. **Glangeaud**: The mountains of Margeride: their porphyric eruptions, cycles of erosion, and glaciers.—J. **Savornin**: The distribution and direction of the phosphate basins in western Morocco. The deposits of Beni Meskine and Oulad Delim are rich, very extensive, and of great commercial importance. The other basins described are only of theoretical interest.—A. **Lumière**: The harmful action of dead leaves on germination. A study of the substances extracted from dead leaves by water showed the presence of reducing substances capable of preventing the germination of seeds. Numerous successive extractions were required before these substances were completely removed. From these observations it is supposed that after the fall of the leaves rain-water extracts these harmful substances during the winter, thus partially sterilising the soil and removing oxygen, and it is only after the lapse of a certain period of time that sufficient oxygen is present in the soil to permit the germination of seeds.—G. **Tanret**: The presence of quinic acid in the leaves of some conifers. The leaves of the cedar collected in July contain 5 gm. of quinic acid per kg. of dried leaves. Similar results were obtained with larch leaves, but the examination of other conifers gave no quinic acid.—A. **Magnan**: The action of the water on the body and head of diving birds.—C. **Dekhuyson**: The biological semi-permeability of the external walls of the Sipunculideæ. Experiments are described which show that the external walls of *Phascolosoma vulgare* and *Sipunculus nudus* are semi-permeable in so far as pure water, carbon dioxide, and oxygen pass through with much greater velocity than salts in solution.—P. **Goy**: The lower plants and the accessory factors of their growth. From studies of the growth under varying conditions of several species of yeasts, moulds, and bacilli, the conclusion is drawn that vitamines are not indispensable to the lower plants, but that their evolution is considerably influenced by the presence of an organic body which has been isolated in a pure crystallisable state from cultures of *Mucor mucedo*. This substance is not an amino-acid, and contains neither nitrogen nor phosphorus.—H. **Bierry** and F. **Rathery**: Diabetes and glycemia.

Books Received.

Klimatographie von Osterreich. VII.: Klimatographie der Bukowina. By Dr. V. Conrad. Pp. 42. VIII.: Klimatographie von Mähren und Schlesien. By H. Schindler. Pp. 125. IX.: Klimatographie von Oberösterreich. By P. T. Schwarz. Pp. 153. (Wien: Gerold & Co.)

Annuaire de l'Observatoire Royal de Belgique, 1922. Pp. vi+316. (Bruxelles.)

The Flowering Plants of South Africa. Edited by Dr. I. B. Pole Evans. Vol. i., No. 2, February. Plates 11–20. (London: L. Reeve and Co., Ltd.; Johannesburg and Cape Town: Speciality Press, Ltd.) Coloured, 15*s.*; plain, 10*s.*

The Theory of Relativity. By Prof. R. D. Carmichael. Second edition. Pp. 112. (New York: J. Wiley and Sons, Inc.; London: Chapman and Hall, Ltd.) 8*s.* 6*d.* net.

Air Ministry, Meteorological Office. International Meteorological Conference. Report of Proceedings of the Fourth International Conference of Directors of Meteorological Institutes and Observatories and of the International Meteorological Committee, Paris, 1919. (M.O. 239.) Pp. 84. (London: Meteorological Office.)

Report of the Secretary of the Smithsonian Institution for the Year ending June 30, 1920. (Publication 2586.) Pp. 110+i plate. (Washington: Government Printing Office.)

Department of the Interior. Bureau of Education. Bulletin No. 65, 1919: The Eyesight of School Children: Defective Vision as Related to School Environment, and Methods of Prevention and Correction. By J. H. Berkowitz. Pp. v+128+xvi plates. Bulletin No. 7, 1920: Requirements for the Bachelor's Degree. By W. C. John. Pp. v+313. Bulletin No. 29: The National Crisis in Education: An Appeal to the People. Edited by W. T. Bawden. Pp. 191. (Washington: Government Printing Office.)

Smithsonian Institution. Bureau of American Ethnology. Bulletin 67: Alsea Texts and Myths. By L. J. Frachtenberg. Pp. 304. (Washington: Government Printing Office.)

Truth about Venereal Disease. By Dr. Marie C. Stopes. Pp. vii+52. (London: G. P. Putnam's Sons.) 1*s.* 6*d.* net.

Ministry of Finance, Egypt. Note on the Programme and Policy of the Government with Regard to the Investigation and Development of the Petroleum Resources of Egypt. By E. M. Dowson. Pp. v+48. (Cairo: Government Press.) P.T. 10.

Transactions of the Royal Society of South Africa. Vol. ix., part 2, 1921. (Cape Town: The Society; London: W. Wesley and Son.) 18s. 6d.

Products of the Empire. By L. Clinton Cunningham. Pp. 299. (Oxford: Clarendon Press.) 5s. 6d. net.

Why do We Die? By T. Bodley Scott. Pp. 123. (London: T. Fisher Unwin, Ltd.) 6s. net.

Calendario della Basilica Pontificia del Santissimo Rosario in Valle di Pompei per l'Anno 1921. Pp. 224+63. (Valle di Pompei.)

Elements of the Mathematical Theory of Electricity and Magnetism. By Sir J. J. Thomson. Fifth edition. Pp. viii+410. (Cambridge: At the University Press.) 30s. net.

The Dynamical Theory of Gases. By Prof. J. H. Jeans. Third edition. Pp. vii+442. (Cambridge: At the University Press.) 30s. net.

The Scientific Papers of Bertram Hopkinson. Collected and arranged by Sir J. Alfred Ewing and Sir Joseph Larmor. Pp. xxvii+480+plates. (Cambridge: At the University Press.) 63s. net.

Handbuch der Anorganischen Chemie. Edited by Prof. R. Abegg and Dr. Fr. Auerbach. Vierter Band. Erste Abteilung. Zweite Halfte. Pp. xiii+1072. (Leipzig: S. Hirzel.) 140 marks.

Agriculture and Irrigation in Continental and Tropical Climates. By Kinsley D. Doyle. Pp. xv+268. (London: Constable and Co., Ltd.) 19s. net.

Instinct in Man: A Contribution to the Psychology of Education. By Dr. J. Drever. Second edition. Pp. x+293. (Cambridge: At the University Press.) 10s. 6d. net.

Proceedings of the Cambridge Philosophical Society. Vol. xx., part 2. (Michaelmas Term, 1920.) Pp. 219-83. (Cambridge.) 5s. net.

An Introduction to Bacterial Diseases of Plants. By Erwin F. Smith. Pp. xxx+688. (Philadelphia and London: W. B. Saunders Co.) 50s. net.

Principles and Methods of Industrial Education. By W. H. Dooley. Pp. xi+257. (London and Sydney: G. G. Harrap and Co., Ltd.) 6s. net.

Psyche's Lamp: A Revelation of Psychological Principles as Foundation of all Thought. By Robert Briffault. Pp. 240. (London: G. Allen and Unwin, Ltd.; New York: The Macmillan Co.) 12s. 6d. net.

Studies in French Forestry. By Theodore S. Woolsey, jun. Pp. xxvi+550. (New York: J. Wiley and Sons, Inc.; London: Chapman and Hall, Ltd.) 30s. net.

Ou en est l'Astronomie. By l'Abbé Th. Moreux. Pp. vi+295. (Paris: Gauthier-Villars et Cie.)

Annuaire pour l'An 1921 publié par le Bureau des Longitudes. Pp. viii+710+A42+B17+C69. (Paris: Gauthier-Villars et Cie.) 8 francs net.

Memoirs of the Connecticut Academy of Arts and Sciences. Vol. vii.: The Appendages, Anatomy, and Relationships of Trilobites. By Dr. Percy E. Raymond. Pp. 169+xi plates. (New Haven, Conn.: Yale University Press.) 6 dollars.

A Research on the Eucalypts, Especially in Regard to their Essential Oils. By Richard T. Baker and Henry G. Smith. Second edition. Pp. xv+471+plates. (Sydney: Technological Museum.)

Perspective: The Practice and Theory of Perspective as Applied to Pictures, with a Section dealing with its Application to Architecture. By Rex Vicat Cole. Pp. viii+17-279. (London: Seeley, Service and Co., Ltd.) 18s. net.

A Diplomat in Japan: The Inner History of the Critical Years of Japan when the Ports were Opened and the Monarchy Restored. By the Right Hon. Sir Ernest Satow. Pp. 427. (London: Seeley, Service and Co., Ltd.) 32s. net.

The Copernicus of Antiquity (Aristarchus of Samos). By Sir Thomas Heath. (Pioneers of Progress: Men of Science.) Pp. iii+59. (London: S.P.C.K.; New York: The Macmillan Co.) 2s. 6d. net.

Early Annals of Ornithology. By J. H. Gurney. Pp. viii+240. (London: H. F. and G. Witherby.) 12s. 6d. net.

Ministry of Finance, Egypt. Survey Department. Meteorological Report for the Year 1913. Pp. xiv+240+xii. (Cairo: Government Press.) P.T.30.

Imperial Mineral Resources Bureau. The Mineral Industry of the British Empire and Foreign Countries (War Period). Antimony (1913-19). Pp. 34. (London: H.M. Stationery Office.) 1s. net.

An Introduction to Technical Electricity. By S. G. Starling. Pp. xii+181. (London: Macmillan and Co., Ltd.) 3s. 6d.

A Book of Gardening for the Sub-Tropics. By Mary Stout and Madeline Agar. Pp. 200. (London: H. F. and G. Witherby.) 6s. net.

Bergens Museums Aarbok, 1918-19. I Hefte. Naturvidenskabelig Roekke. Pp. 76+37+86+vii plates. (Bergen.)

An Account of the Crustacea of Norway. By G. O. Sars. Vol. vii.: Copepoda. Supplement. Parts vii. and viii.: Harpacticoida (continued). Pp. 73-92+xlix-lxiv plates. (Bergen: Bergen Museum.)

Diary of Societies.

THURSDAY, FEBRUARY 17.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Prof. W. A. Herdman: Oceanography (The Sea-Fisheries).

ROYAL SOCIETY, at 4.30.—Dr. C. Chree: A Comparison of Magnetic Declination Changes at British Observatories.—Prof. H. M. Macdonald: The Transmission of Electric Waves Around the Earth's Surface.—Prof. T. H. Havelock: The Stability of Fluid Motion.—Prof. W. H. Young: The Transformation of Integrals.—J. L. Haughton and Kathleen E. Bingham: The Constitution of the Alloys of Aluminium, Copper, and Zinc containing High Percentages of Zinc.

ROYAL COLLEGE OF PHYSICIANS OF LONDON, at 5.—Dr. Martin Flack: Respiratory Efficiency in Relation to Health and Disease (Milroy Lecture).

LINNEAN SOCIETY, at 5.—Prof. G. B. De Toni: A Contribution to the Teratology of the Genus Datura.—Capt. J. Ramsbottom and A. J. Wilmott: A Plant Collection from Macedonia.—Dr. G. C. Druce: Shetland *Plantago* and Other Plants from the Northern Isles.

ROYAL SOCIETY OF MEDICINE (Dermatology Section), at 5.

ROYAL AERONAUTICAL SOCIETY (at Royal Society of Arts), at 5.30.—F. Handley Page: The Handley Page Wing.

INSTITUTION OF MINING AND METALLURGY (at Geological Society), at 5.30.—E. H. Clifford: A Scheme for Working the City Deep Mine at a Depth of 7000 ft.

INSTITUTION OF ELECTRICAL ENGINEERS (at Institution of Civil Engineers), at 6.—Prof. E. Wilson: Magnetic Susceptibility of Low Order: I. Instrumentation.

INSTITUTION OF AUTOMOBILE ENGINEERS (London Graduates' Meeting) (at 28 Victoria Street), at 7.30.—E. L. Bass: Engine Lubrication.

CHEMICAL SOCIETY, at 8.—L. J. Hudleston and H. Bassett: Equilibria of Hydrofluosilicic Acid. Part I. Mixtures of Hydrofluosilicic and Hydrofluoric Acids.—R. G. Fargher and H. King: Additive Compounds of Antipyrylaminodiacetic Acid and its Salts with Neutral Salts.—H. Bassett and T. A. Simmons: The System, Picric Acid—Phenyl Acridine.—F. S. Kipping: Organic Derivatives of Silicon. Part XXIV. *dl*-Derivatives of Silicoethane.—F. W. Atack: The Structural Isomerism of Oximes. Part I. Criticism of the Hantzsch-Werner Hypothesis of the Geometrical

Isomerism of Carbon-Nitrogen Compounds.—F. W. Atack: The Structural Isomerism of Oximes. Part II. Constitution of Oximes.—F. S. Kipping: Organic Derivatives of Silicon. Part XXV. Saturated and Unsaturated Silicohydrocarbons, Si_4Ph_8.

RÖNTGEN SOCIETY (at University College), at 8.15.—N. E. Luboshez: Intensifying Screens and Secondary Radiation.

FRIDAY, FEBRUARY 18.

ASSOCIATION OF ECONOMIC BIOLOGISTS, Annual General Meeting (in Botanical Lecture Theatre, Imperial College of Science), at 2.30.—Sir David Prain: Some Relationships of Economic Biology.

GEOLOGICAL SOCIETY OF LONDON, at 3.—Anniversary Meeting.

ROYAL SOCIETY OF ARTS (Indian Section), at 4.30.

ROYAL SOCIETY OF MEDICINE (Otology Section), at 5.—Dr. A. R. Friel: Zinc Ionisation and Electrolysis in the Treatment of Chronic Otorrhœa.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—J. F. Dobson: The Function of the Kidneys in Enlargement of the Prostate Gland (Arris and Gale Lecture).

INSTITUTION OF MECHANICAL ENGINEERS, at 6.—Annual General Meeting.—F. M. Farmer: The Desirability of Standardisation in the Testing of Welds.

INSTITUTION OF ELECTRICAL ENGINEERS (Students' Section) (at King's College, Strand), at 6.30.—L. B. Atkinson: Presidential Address.

ROYAL SOCIETY OF TROPICAL MEDICINE AND HYGIENE (at 11 Chandos Street, W.1), at 8.15.—Lt.-Col. J. O. Kennedy: Pathology of Relapsing Fever.—Dr. A. M. Cole: Five Cases of Sasciolopsis Infection.—Dr. A. Connall: Examination of Chrysops for Filaria in West Africa.

ROYAL SOCIETY OF MEDICINE (Electro-Therapeutics Section), at 8.30.

ROYAL INSTITUTION OF GREAT BRITAIN, at 9.—S. J. Solomon: Strategic Camouflage.

SATURDAY, FEBRUARY 19.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Prof. A. Fowler: Spectroscopy (Regularity in Spectra).

MONDAY, FEBRUARY 21.

VICTORIA INSTITUTE (at Central Hall, Westminster), at 4.30.—Lt.-Col. F. A. Molony: Prophecy.

ROYAL COLLEGE OF SURGEONS OF ENGLAND, at 5.—J. H. Evans: The Azygos System of Veins in their Association with Surgical Affections (Arris and Gale Lecture).

ARISTOTELIAN SOCIETY (at University of London Club, 21 Gower Street), at 8.—Miss H. D. Oakeley and Others: Space, Time, and Deity.

ROYAL SOCIETY OF ARTS, at 8.—Dr. E. K. Rideal: Applications of Catalysis to Industrial Chemistry: Processes of Oxidation (Cantor Lecture).

CHEMICAL INDUSTRY CLUB (at 2 Whitehall Court), at 8.—Lt.-Col. W. Cullen: South Africa.

ROYAL GEOGRAPHICAL SOCIETY (at Æolian Hall), at 8.30.—Dr. H. Rice: The Rio Negro Casiquiare Canal and Upper Orinoco.

MEDICAL SOCIETY OF LONDON (at 11 Chandos Street, W.1), at 9.—G. E. Gask: Surgery of the Lung and Pleura (Lettsomian Lecture).

TUESDAY, FEBRUARY 22.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Prof. A. Keith: Darwin's Theory of Man's Origin, in the Light of Present Day Evidence.

ROYAL HORTICULTURAL SOCIETY, at 3.

ROYAL SOCIETY OF MEDICINE (Medicine Section) (at Guy's Hospital), at 4.—Dr. G. H. Hunt: An Experimental Oxygen Chamber.—Dr. G. Marshall: Apparatus for Induction of Artificial Pneumothorax.—Dr. G. H. Hunt and Dr. E. P. Poulton: Congenital Heart Disease with Analysis of the Arterial Blood.—Dr. C. P. Symonds: A Case of Charcot-Marie-Tooth Muscular Dystrophy with Pseudo Hypertrophy.—Dr. G. H. Hunt: Cardiac Efficiency in Normal and Pathological Conditions.—J. M. H. Campbell: Factors in the Causation of Dyspnœa.—Dr. J. A. Ryle: The Diagnostic Value and the Limitations of the Fractional Test-meal.

ROYAL COLLEGE OF PHYSICIANS OF LONDON, at 5.—Dr. Martin Flack: Respiratory Efficiency in Relation to Health and Disease (Milroy Lecture).

ZOOLOGICAL SOCIETY OF LONDON, at 5.30.—Dr. P. Chalmers Mitchell: Report on the Additions to the Society's Menagerie during the Month of January, 1921.—Prof. Elliot Smith: Exhibition of Photographs of Live Examples of Tarsius.—A. Mallock: Colour-production in Relation to the Coloured Feathers of Birds.—E. Dukinfield Jones: Descriptions of New Moths from South-East Brazil.—Dr. J. Stephenson: Morphology, Classification, and Zoogeography of Indian Oligochæta.—Dr. N. S. Lucas: Report on Deaths in the Society's Gardens during the Year 1920.—Dr. R. Broom: The Structure of the Reptilian Tarsus.

INSTITUTION OF CIVIL ENGINEERS, at 5.30.—P. Allan: Port Improvements at Newcastle, N.S.W.

INSTITUTION OF PETROLEUM TECHNOLOGISTS (at Royal Society of Arts), at 5.30.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN, at 7.—D. Charles: Improvements in Flashlight.

ROYAL ANTHROPOLOGICAL INSTITUTE (at Royal Society), at 8.15.—Sir Alfred T. Davies: The Scheme of the Welsh Department of the Board of Education for the Collection of Rural Lore in Wales through the Schools. To be followed by a Discussion.

WEDNESDAY, FEBRUARY 23.

ROYAL SOCIETY OF ARTS, at 4.30.—Sir Daniel Hall: The Present Position of Research in Agriculture (Trueman Wood Lecture).

GEOLOGICAL SOCIETY OF LONDON, at 5.30.—Prof. W. J. Sollas: *Saccammina Carteri*, Brady, and the Minute Structure of the Foraminiferal Test.—Dr. T. S. Wilson: Notes on the Views of the Late Prof. C. Lapworth with Regard to Spiral Movements in Rocks during Elevation or Depression.

INSTITUTION OF CIVIL ENGINEERS (Students' Meeting), at 6.—J. H. Barker: Machinery Applied to Mass Production.

THURSDAY, FEBRUARY 24.

MEDICO-PSYCHOLOGICAL ASSOCIATION OF GREAT BRITAIN AND IRELAND (at 11 Chandos Street, W.1), at 2.45.—Sir Frederick Mott and Dr. Hayao: The Pathology of Dementia Præcox, especially in Relation to the Condition of the Ovaries.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—F. Balfour Browne: Mason Bees and Wasps.

ROYAL SOCIETY, at 4.30.—*Probable Papers.*—Sir Ray Lankester: A Remarkable Flint-implement from Selsey Bill.—Dr. E. J. Allen: Regeneration and Reproduction of the Syllid Procerastea.—E. C. Grey and E. G. Young: The Enzymes of *B. coli communis*. Part II. (*a*) Anaerobic Growth followed by Anaerobic and Aerobic Fermentation. (*b*) The Effects of Aeration during the Fermentation.—Dr. A. E. Everest and A. J. Hall: Anthocyanins and Anthocyanidins.

ROYAL SOCIETY OF MEDICINE (Tropical Medicine Section), at 5.—Sir Leonard Rogers: Presidential Address.—Dr. J. G. Thomson and Dr. A. Robertson: The Value of Laboratory Reports in the Diagnosis of Suspected Dysentery, and their Interpretation by the Clinician.

ROYAL COLLEGE OF PHYSICIANS OF LONDON, at 5.—Dr. Martin Flack: Respiratory Efficiency in Relation to Health and Disease (Milroy Lecture).

INSTITUTION OF ELECTRICAL ENGINEERS (at Institution of Civil Engineers), at 6.—Prof. E. Wilson: Magnetic Susceptibility of Low Order. II. Susceptibility Values.

ILLUMINATING ENGINEERING SOCIETY (at Royal Society of Arts), at 8.—Discussion: The Use of Light as an Aid to Publicity.

ROYAL SOCIETY OF MEDICINE (Urology Section), at 8.30.

FRIDAY, FEBRUARY 25.

ROYAL SOCIETY OF MEDICINE (Study of Disease in Children Section), at 4.30.—Sir Humphry Rolleston and Others: Discussion on The Diagnosis and Treatment of Congenital Syphilis and its Effects.

PHYSICAL SOCIETY OF LONDON (at Imperial College of Science), at 5.—R. H. Humphry: A Note on the Hot-Wire Inclinometer.—Profs. E. Wilson and Herroun: The Magnetic Susceptibility of Certain Natural and Artificial Oxides.—J. Guild: The Refractometry of Prisms.—T. Smith: Tracing Rays through an Optical System.

ROYAL SOCIETY OF MEDICINE (Epidemiology and State Medicine Section), at 8.30.—Dr. R. M. F. Picken: The Epidemiology of Measles in a Rural and Residential Area.

SATURDAY, FEBRUARY 26.

ROYAL INSTITUTION OF GREAT BRITAIN, at 3.—Prof. A. Fowler: Spectroscopy (Celestial Spectroscopy).

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